



L. R. 1.





Richard Taylor

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"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condant,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.



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ERRATA.

- Page 361, line 18 from bottom, a full point after the word electricity, delete *if*, and put A for a.
- 362, line 13 from top, *for* cloudy *read* cloudless.
- 363, line 19 from bottom, *for* out a window *read* out of a window.

PLATES.

FRONTISPIECE to the Volume—Portrait of RICHARD TAYLOR, F.L.S.

- I. Illustrative of Mr. C. J. Burnett's Paper on several Forms of Actinometer.
- II. Illustrative of Professors Kirchhoff and Bunsen's Paper on Chemical Analysis by Spectrum-observations.
- III. Illustrative of Dr. J. H. Gladstone's Paper on the Electric Light of Mercury, and Mr. T. Tate's Paper on a new Self-registering Mercury Barometer.
- IV. Illustrative of M. F. August's Paper on a new Species of Stereoscopic Phenomenon, Prof. Dove's Paper on the Dichroscope, and Mr. T. Tate's Paper on the Laws of Absorption of Liquids by Porous Substances.

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[FOURTH SERIES.]

JULY 1860.

I. *On the Relation between the Radiating and Absorbing Powers of different Bodies for Light and Heat.* By G. KIRCHHOFF*.

A BODY placed within a covering whose temperature is the same as its own, is unaffected by radiation, and must therefore absorb as many rays as it emits. Hence it has long been concluded that, at the same temperature, the ratio between the radiating and absorbing powers of all bodies is the same,—it being, however, assumed that bodies only emit rays of one kind. This law has been verified experimentally, especially by MM. de la Provostaye and Desains, in many cases in which the homogeneousness of the emitted rays could at least be so far assumed, inasmuch as they were all invisible. Whether the same law holds good when bodies emit rays of different kinds (which, strictly speaking, is always the case), has never hitherto been determined theoretically or by experiment. I have, however, now found that the law in question extends to this case also, provided that by the radiating power the intensity of one species of emitted rays be understood, and that the absorbing power be estimated with reference to rays of the same kind. Taken in this way, the ratio of the radiating and absorbing powers of all bodies at the same temperature is the same. I shall first give the theoretical proof of this principle, and then develope certain remarkable consequences that immediately follow therefrom, which partly explain phænomena already known, and partly suggest new ones.

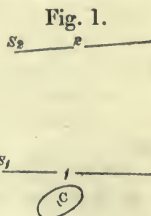
All bodies emit rays, the quality and intensity of which depend on the nature and temperature of the bodies themselves. In addition to these, however, there may, under certain circumstances, be rays of other kinds,—as, for example, when a body

* Translated by Mr. F. Guthrie from Poggendorff's *Annalen*, vol. cix. p. 275.

is sufficiently charged with electricity, or when it is phosphorescent or fluorescent. Such cases are, however, here excluded.

When a body encounters rays from without, it absorbs a portion of them and converts it into heat. But, besides this species of absorption, there may, under certain circumstances, be others, as, for example, when a body is phosphorescent or fluorescent. It is, however, here assumed that all absorbed rays are converted into heat.

§ 1. Before the body C (fig. 1), imagine two screens, S_1 , S_2 , to be placed, containing openings 1 and 2, whose dimensions must be regarded as infinitely small in comparison with their distance apart, and each of which has a middle point. Through these two openings a pencil proceeds from the body C. Of this pencil let that part be considered which consists of waves, the length of which lies between λ and $\lambda + d\lambda$, and let this be divided into two component parts polarized in the perpendicular planes a and b passing through the axis of the pencil. Let the intensity of the part polarized in a be $E d\lambda$: E is then the radiating power of the body.



Conversely, a pencil of rays polarized in plane a , and having waves of the length λ , falls on the body C through the openings 2 and 1. Of this, part is absorbed by the body, the rest being partly reflected and partly transmitted. Let the ratio of the absorbed to the incident rays be called A ; then A will represent the power of absorption of the body.

The magnitudes E and A depend on the nature and temperature of C, on the position and form of the openings 1 and 2, on the magnitude λ , and on the position of the plane a . It will be shown that the ratio of E to A is independent of the nature of the body; it will thence necessarily follow that it cannot be affected by the position of the plane a , and its independence of the position and form of the openings 1 and 2 will thence be easily deduced, so that it only remains to be determined how far it depends on the temperature of C and the wave-length λ .

The proof I am about to give of the law above stated, rests on the supposition that bodies can be imagined which, for infinitely small thicknesses, completely absorb all incident rays, and neither reflect nor transmit any. I shall call such bodies *perfectly black*, or, more briefly, *black* bodies. It is necessary in the first place to investigate the radiating power of bodies of this description.

§ 2. Let C be a black body. Let its radiating power (generally indicated by E) be called e . It will be shown that e remains the same when C is replaced by any other black body of the same temperature.

Imagine the body C enclosed in a black covering, of which the screen S_1 forms a part. Let the second screen, S_2 , be black also, and let the two be united all round by black walls (fig. 2). Let the opening 2 be in the first place closed by a black surface, which I shall call surface 2, and let the whole system be kept at a constant temperature by being completely enclosed in a covering impermeable to heat, as, for example, in a perfectly reflecting surface. Then, since the temperature of the body C remains constant, the intensity of the incident rays (which, according to hypothesis, it entirely absorbs) must be equal to that of the emitted rays. Now imagine surface 2 to be removed and replaced by a portion of a perfectly reflecting spherical mirror having its centre in the middle point of opening 1. The equilibrium of the temperature of the system will be undisturbed, and the equality of the intensities of the emitted and incident rays must therefore still subsist. As, however, the body C emits the same rays as before, it follows that the intensity of the rays that impinge on it must be the same in both cases. By the removal of surface 2, those rays are withdrawn from the body which proceeded from that surface through opening 1. In the place of the rays so withdrawn, the mirror applied at 2 reflects back on the body the rays which proceeded from it through the openings 1 and 2*. Whence it may be concluded that the intensity of the pencil which proceeds from the body C through the openings 1 and 2, is equal to the intensity of the pencil which at the same temperature proceeds from surface 2 through opening 1. But this is independent of the form and constitution of the body C. The alleged law is therefore proved when all the rays of the pencils compared have the same wavelength λ , and are polarized in the same plane a . Regard, however, to the possible diversity of these rays renders somewhat more complex considerations necessary.

§ 3. In the arrangement represented in fig. 2, imagine a small plate P (fig. 3) placed between openings 1 and 2, which in the visible rays displays the colours of thin plates, and which, partly on account of its extreme thinness, and partly on account of its material constitution, neither absorbs nor emits any perceptible quantity of rays. Let this plate be so placed that it cuts the pencil passing between 1 and 2 at the

Fig. 2.

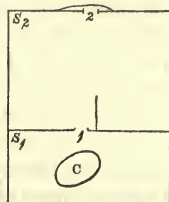
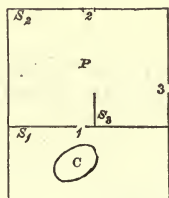


Fig. 3.



* The effect of the diffraction of the rays by the edges of opening 1 is here neglected. This is allowable if openings 1 and 2, though infinitely small in comparison with their distance apart, be considered as very great in comparison with the length of a wave.

4 Prof. Kirchhoff on the Relation between the Radiating and

angle of polarization, the plane of incidence being a . Let the wall which unites S_1 and S_2 be so situated that the image cast by plate P of opening 2 lies in it; and in the place and of the form of this image imagine an opening, which I shall call opening 3. Let opening 2 be closed by a black surface of the same temperature as the rest of the system, and let opening 3 be closed, in the first place by a similar surface (which I shall call surface 3), and secondly, by a perfect concave mirror, having its centre in the image which the plate P casts of opening 1. In both cases the equilibrium of temperature remains undisturbed, and for reasons similar to those mentioned in the last section; it thence follows that the sum of the intensity of the rays withdrawn from the body by the removal of surface 3, is equal to the sum of the intensities of the rays incident on the body in consequence of the application of the mirror. Let a black screen S_3 , of the temperature of the rest of the system, be so placed that no rays emanating from opening 3 can reach the body directly. The first sum is then the intensity of the rays which proceed from surface 3, are reflected by plate P, and pass through the opening 1; let this be indicated by Q. The second sum consists of two parts: one depending on the body C, which is

$$= \int_0^{\infty} d\lambda e r^2,$$

where r indicates a magnitude depending on the constitution of plate P, and independent of λ ; the second part consisting of rays which have proceeded from some portion of the black wall uniting S_1 and S_2 , have penetrated plate P, and have been reflected, first by the mirror, and then by P. This portion I shall indicate by R. It is unnecessary further to determine the value of R, it being sufficient to observe that R, like Q, is independent of the nature of C. Between these magnitudes, then, there subsists the following equation:

$$\int_0^{\infty} d\lambda e r^2 + R = Q.$$

If, now, the body C be replaced by some other black body of the same temperature, e' indicating for this body what e does for the other, then

$$\int_0^{\infty} d\lambda e' r^2 + R = Q.$$

Whence

$$\int_0^{\infty} d\lambda (e - e') r^2 = 0.$$

Now let the index of refraction of plate P be supposed to be

infinitely near unity. From the theory of the colours of thin plates, it follows that

$$r = \rho \sin^2 \frac{p}{\lambda};$$

where p indicates a magnitude proportional to the thickness of the plate, and independent of λ , and ρ a magnitude independent of the thickness of the plate. The former equation then becomes

$$\int_0^\infty d\lambda (e - e') \rho^2 \sin^4 \frac{p}{\lambda} = 0.$$

And since this equation holds good whatever be the thickness of plate P, that is, whatever be the value of p , it may be deduced that whatever λ may be,

$$e - e' = 0.$$

In order to prove this, in the above equation for $\sin^4 \frac{p}{\lambda}$ substitute its value

$$\frac{1}{8} \left(\cos 4 \frac{p}{\lambda} - 4 \cos 2 \frac{p}{\lambda} + 3 \right),$$

and differentiate twice with respect to p , we then have

$$\int_0^\infty d\lambda \frac{e - e'}{\lambda^2} \rho^2 \left(\cos 4 \frac{p}{\lambda} - \cos 2 \frac{p}{\lambda} \right) = 0.$$

Let $\frac{2}{\lambda} = \alpha$ and $(e - e') \rho^2 = f\alpha$. Then

$$\int_0^\infty d\alpha f\alpha (\cos 2p\alpha - \cos p\alpha) = 0.$$

And since when $\phi\alpha$ is any function of α ,

$$\int_0^\infty d\alpha \phi\alpha \cos 2p\alpha = \frac{1}{2} \int_0^\infty d\alpha \phi\left(\frac{\alpha}{2}\right) \cos p\alpha,$$

as will be seen if $\frac{\alpha}{2}$ be substituted for α , the above equation may be written as follows:

$$\int_0^\infty d\alpha \left(f \frac{\alpha}{2} - 2f\alpha \right) \cos p\alpha = 0.$$

Multiply this equation by $dp \cos px$, where x is any magnitude whatever, and integrate from $p=0$ to $p=\infty$. Then by Fourier's formula, that

$$\int_0^\infty dp \cos px \int_0^\infty d\alpha \phi(\alpha) \cos p\alpha = \frac{\pi}{2} \phi\lambda,$$

we get

$$f\left(\frac{x}{2}\right) = 2fx,$$

or

$$f\left(\frac{\alpha}{2}\right) = 2f\alpha.$$

From which it follows, either that $f\alpha$ is nothing for every value of α , or that it is infinitely great when α vanishes. But when α vanishes, λ becomes infinite. Recollecting then the meaning of $f\alpha$, and recollecting also that ρ is a proper fraction, and that neither e nor e' can become infinite when λ increases without limit, the second alternative cannot be admitted, and therefore for every value of λ we must have

$$e = e'.$$

§ 4. If the pencil which proceeds from the black body C through the openings 1 and 2 consisted partly of rays polarized in a plane, the plane of polarization of the polarized portion must rotate when the body itself rotates about the axis of the pencil. Such a rotation must therefore affect the value of e . But since, by the above equation, no such effect can be admitted, it follows that no part of the pencil can be so polarized. It can also be shown that no part of the pencil can be circularly polarized. We shall not give the proof of this here. Without this it will be admitted that black bodies can be imagined so constituted that there is no more reason why they should emit rays circularly polarized in one direction more than the other. All the black bodies hereafter mentioned are supposed to be of this kind, viz. that they emit no polarized rays whatever.

§ 5. The magnitude indicated by e depends, not only on the temperature and length of the wave, but also on the form and relative position of the openings 1 and 2. Let w_1 and w_2 be the projections of these openings on the planes which cut the pencil at right angles to its axis, and let s be the distance of the openings apart; then

$$e = I \frac{w_1 w_2}{s_2},$$

where I is a function of the length of the wave and the temperature only.

§ 6. As the form of the body C is arbitrary, we may substitute for it a surface which exactly fills the opening 1, and which we shall call surface 1. The screen S_1 may then be imagined to be removed. The screen S_2 may also be removed, if the pencil to which e relates be defined as that which proceeds from surface 1 and is incident on surface 2, which exactly fills the opening 2.

§ 7. A consequence that immediately follows from the equa-

tion last obtained, and which will afterwards be made use of, is that openings 1 and 2 may be interchanged.

§ 8. We shall now establish a law which may be regarded as a generalization of that announced in the last section.

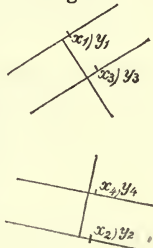
Between the two black surfaces 1 and 2 of equal temperature, imagine a body placed which may refract, reflect, and absorb the rays which pass between them in any way whatever. Several pencils may reach surface 2 from surface 1; of these let one be chosen, and let that part of it be taken when it leaves 1, which consists of waves of length between λ and $\lambda + d\lambda$, and let this be divided into two component parts polarized at right angles to each other in the planes a_1 and b_1 . Let that part of the first component which reaches 2 be itself divided into two parts, whose planes of polarization are the perpendicular but otherwise arbitrary planes a_2, b_2 . Let the intensity of the part polarized in a_2 be $Kd\lambda$. Of the pencil which pursues the same path, but in the opposite direction, viz. from 2 to 1, consider at 2 the part which consists of the waves whose length lies between λ and $\lambda + d\lambda$, and let it again be divided into two parts polarized in the planes a_2 and b_2 , and let the portion of the first component part that reaches 1 be also divided into two components polarized in a_1 and b_1 . Let the intensity of the part polarized in a_1 be $K'd\lambda$. Then

$$K = K'.$$

The truth of this proposition shall, in the first place, be established on the hypothesis, first, that the rays suffer no diminution of intensity on their path, that is, that the refractions and reflexions to which they may be subjected cause no loss, and that there is no absorption; and secondly, that the rays that proceed from 1 polarized in a_1 , impinge on 2 polarized in a_2 , and conversely.

Through the middle point of 1 let a plane be placed perpendicular to the axis of the incident and emerging pencil, and in this plane let a system of rectangular coordinates be taken, of which the middle point of 1 is the origin. Let x_1, y_1 be the coordinates of some point in this plane (fig. 4). At the distance of unity from this plane let another plane be taken parallel to the first, and containing a system of coordinates parallel to the first system, having its origin in the axis of the pencil. Let x_3, y_3 be the coordinates of a point in this plane. Similarly, let a plane be taken passing through the middle point of 2, perpendicular to the axis of the incident and emerging pencil, and in this plane let a system of rectangular coordinates be taken having the middle point of 2 for its origin. Let x_2, y_2 be the coordinates of a point in this

Fig. 4.



plane. Lastly, at the distance of unity from the last plane, and parallel to it, let a fourth plane be taken, containing a system of rectangular coordinates parallel to those in the third plane, and having the axis of the pencil for its origin. Let x_4, y_4 be the coordinates of any point in this plane.

From the point x_1y_1 a ray is supposed to proceed to the point x_2y_2 . Let the time it takes to pass from one point to the other be called T. Then T is some function of x_1y_1, x_2y_2 , which we will suppose to be known. If the points x_3y_3, x_4y_4 lie in the path of the ray, and if for the sake of brevity the velocity of the ray *in vacuo* be taken as unity, then the time required to pass from x_3y_3 to x_4y_4 will be

$$= T - \sqrt{1 + x_1 - x_3|^2 + y_1 - y_3|^2} \\ - \sqrt{1 + x_2 - x_4|^2 + y_2 - y_4|^2}.$$

If the points x_3y_3, x_4y_4 were given, and the points x_1y_1, x_2y_2 were required, they might be found from the condition that the above expression is a minimum. Supposing, therefore, that the eight coordinates, $x_1, y_1, x_2, y_2, x_3, y_3, x_4, y_4$, are very small, the condition that the four points of which they are the coordinates shall all lie in the path of the same ray is expressed by the following equations:—

$$x_3 = x_1 - \frac{\partial T}{\partial x_1}, \quad x_4 = x_2 - \frac{\partial T}{\partial x_2}, \\ y_3 = y_1 - \frac{\partial T}{\partial y_1}, \quad y_4 = y_2 - \frac{\partial T}{\partial y_2}.$$

Now let x_1y_1 be a point in the projection of surface 1 on the plane x_1y_1 , and let $dx_1 dy_1$ be the element of this projection which contains the point x_1y_1 , and which must be considered as infinitely small as compared with the surfaces 1 and 2. Let x_3y_3 be a point in a ray which proceeds from 1 to 2, $dx_3 dy_3$ the superficial element containing the point x_3y_3 , and of the same order of magnitude as $dx_1 dy_1$. The intensity of the rays whose waves are of the length already mentioned, and which are polarized in the given plane, and proceed from x_1y_1 through x_3y_3 , is then, according to § 5,

$$d\lambda I dx_1 dy_1 dx_3 dy_3.$$

Now according to the hypothesis we have assumed, the pencil arrives at 2 with its intensity undiminished, and forms an element of the magnitude indicated by $Kd\lambda$. Whence for K we must have the integral

$$I \iiint dx_1 dy_1 dx_3 dy_3$$

taken between the proper limits.

The integration according to x_3 and y_3 must be between the

values which those magnitudes have according to the equations above obtained, while x_1 and y_1 are constant, and x_2y_2 have all the different values which answer to the different points of the projection of surface 2 on the plane x_2y_2 ; the integrals according to x_1y_1 must then be taken over the projection of surface 1. The double integral,

$$\iint dx_3 dy_3,$$

so limited is

$$= \iint \left(\frac{\partial x_3}{\partial x_2} \frac{\partial y_3}{\partial y_2} - \frac{\partial x_3}{\partial y_2} \frac{\partial y_3}{\partial x_2} \right) dx_2 dy_2;$$

or applying the equations for x_3 and y_3 ,

$$= \iint \left(\frac{\partial^2 T}{\partial x_1 \partial x_2} \cdot \frac{\partial^2 T}{\partial y_1 \partial y_2} - \frac{\partial^2 T}{\partial x_1 \partial y_2} \cdot \frac{\partial^2 T}{\partial y_2 \partial x_1} \right) dx_2 dy_2,$$

where the integrals are to be taken over the projection of surface 2. Whence

$$K = I \iiint \left(\frac{\partial^2 T}{\partial x_1 \partial x_2} \cdot \frac{\partial^2 T}{\partial y_1 \partial y_2} - \frac{\partial^2 T}{\partial x_1 \partial y_2} \cdot \frac{\partial^2 T}{\partial y_2 \partial x_1} \right) dx_1 dy_1 dx_2 dy_2,$$

where the integrals are to be taken over the projections of surfaces 1 and 2.

If the magnitude K' be treated in the same way, it being remembered that a ray takes the same time to pass between two points down the same path in either direction, the same expression will be found for K' as for K . The proposition to be proved is thus demonstrated, subject to the limitation already mentioned. This limitation may, however, be got rid of by means of an observation made by Helmholtz in his 'Physical Optics,' p. 169. Helmholtz here says (with somewhat different notation), "A ray of light proceeding from point 1 arrives at point 2 after suffering any number of refractions, reflexions, &c. At point 1 let any two perpendicular planes a_1, b_1 be taken in the direction of the ray; and let the vibrations of the ray be divided into two parts, one in each of these planes. Take similar planes a_2, b_2 in the ray at point 2; then the following proposition may be demonstrated. If when the quantity i of light polarized in the plane a_1 proceeds from 1 in the direction of the given ray, the part k thereof of light polarized in a_2 arrives at 2, then, conversely, if the quantity i of light polarized in a_2 proceeds from 2, the same quantity k of light polarized in a_2 will arrive at 1*."

* This proposition of Helmholtz ceases to hold good, as he himself observes, when the plane of polarization of the ray suffers any alteration such as that produced by magnetism, according to Faraday's discovery. In what follows, therefore, the effect of magnetic force must be excluded. Helmholtz limits his proposition also by the supposition that light suffers

Applying this proposition, and representing by γ the ratio $\frac{k}{i}$, in whichever direction the ray passes between the points x_1y_1 and x_2y_2 , an expression is obtained for K and K' which only differs from that above obtained by the occurrence of γ as a factor under the integral sign.

The equality of K and K' therefore still subsists, even when γ has different values in the rays into which any one of the compared pencils may be considered as divided; it is, for example, unaffected if any part of the pencil be intercepted by a screen.

§ 9. The following proposition may also be proved of the same pencils as were compared in the last section. Of the pencil which proceeds from 1 to 2, consider at 2 that part which consists of waves whose length lies between λ and $\lambda d\lambda$, and let it be divided into two components polarized in a_2 and b_2 . Let the intensity of the first of these components be $Hd\lambda$. Of the pencil that proceeds from 2 to 1, consider at 2 the part consisting of waves whose length lies between λ and $\lambda + d\lambda$, and divide this into two parts polarized in a_2 and b_2 . Let the intensity of that portion of the first part which arrives at 1 be $H'd\lambda$. Then must

$$H = H'.$$

The proof of this proposition is as follows:—Let K and K' have the same meaning as in the previous section, L and L' being the magnitudes that K and K' become when planes a_1 and b_1 are interchanged. Then $L = L'$, just as $K = K'$, and also

$$H = K + L;$$

for rays polarized perpendicularly to each other, provided they are parts of a non-polarized ray, do not interfere when they are brought back to a common plane of polarization; and, according to § 4, surface 1 emits none but non-polarized rays.

Lastly, we must have

$$H' = K' + L',$$

because two rays, whose planes of polarization are perpendicular, do not interfere. From these equations it follows that $H = H'$.

§ 10. Let fig. 2 have the same meaning as in § 3; only let the body C be no longer black, but a body of any kind. Let opening 2 be closed by surface 2; then a pencil proceeding from this surface through opening 1 reaches C , and is there partly absorbed and partly dispersed in various directions by reflexion and refraction. Of this pencil between 1 and 2 let that part be considered

no change of refrangibility such as occurs in fluorescence; this limitation, however, ceases to be necessary if, in the application of the proposition, only rays of a given length of wave are regarded.

which consists of waves whose length lies between λ and $\lambda + d\lambda$, and let it be divided into two components polarized in a and its perpendicular. Let that part of the first component which is not absorbed by C, and which therefore reaches the black covering in which the body C is enclosed, be $M'd\lambda$. Of the rays that proceed from the covering and are incident on C, a certain portion reach surface 2 through opening 1. The body C thus originates a pencil of rays which is incident on surface 2 through opening 1. Of this pencil consider that part which consists of waves whose length lies between λ and $\lambda + d\lambda$, and let this be divided into two parts polarized in plane a and its perpendicular. Let the intensity of the first component be $Md\lambda$. Then is

$$M = M'.$$

This follows from the proposition established in the last section, by applying that proposition to all the pencils which surface 2 and all the elements of the black covering exchange with each other through the medium of the body C, and then summing the equations so obtained.

§ 11. Let the arrangement represented in fig. 3, and described in § 3, be again taken, C, however, being no longer a black body, but one of any kind. In both the cases described in that section the equilibrium of temperature must still subsist, and therefore the *vis viva* that is withdrawn from the body by the removal of the surface 3, will be equal to the *vis viva* it receives by the application of the concave mirror. Let the letters used in § 3 have the same meaning as in that section, and let E and A have the signification given them in § 1. Then the *vis viva* withdrawn from the body C by the removal of surface 3 is, according to § 7,

$$= \int_0^{\infty} d\lambda \epsilon r A.$$

The *vis viva* which the body receives by means of the concave mirror consists of three parts:—The first due to the rays emitted by C itself; this is

$$= \int_0^{\infty} d\lambda E r^2 A.$$

The second due to the rays which, having proceeded from the part of the black covering opposite the mirror, have passed through the plate P, and have been reflected, first by the mirror, and then again by P. This, according to § 9, is

$$= \int_0^{\infty} d\lambda \epsilon r (1 - r) A.$$

The third and last part is due to the rays which have fallen on C

from various parts of the black covering, have been thence reflected or refracted through opening 1 towards surface 2, have been reflected by the plate P, and again by the mirror at 3, and, lastly, again reflected by P through opening 1. If M then have the meaning given it in § 10, this last part

$$= \int_0^{\infty} d\lambda M r^2 A.$$

It may appear doubtful whether the above expressions for the first and third of these portions are correct when C is in such a position that a finite portion of the pencil proceeding from 2 through opening 1, and incident on C, is by C reflected back towards 2. Such cases are therefore for the present excluded.

According to § 10, $M = M'$, and by definition $M' = e(1 - A)$. The third part is therefore

$$= \int_0^{\infty} d\lambda e(1 - A) r^2 A,$$

whence we have the equation

$$\int_0^{\infty} d\lambda (E - Ae) Ar^2 = 0.$$

And from considerations identical with those mentioned in § 3 with reference to a similar equation, the conclusion may be drawn, that for every value of λ

$$\frac{E}{A} = e;$$

or, putting for e its value as obtained in § 5,

$$\frac{E}{A} = I \frac{w_1 w_2}{s^2}.$$

The proposition we undertook to prove is therefore established, subject to the condition that no finite part of the pencil that proceeds from surface 2 through opening 1, and is incident on the body C, is reflected back by C to surface 2. That the proposition is true without this limitation, is obvious when we consider that, if the condition in question be not fulfilled, it is only necessary that the body C should be turned through an infinitely small angle in order to satisfy it, and that such a change of position can only cause an infinitely small change in the values of A and E.

The magnitude indicated by I is, as remarked in § 5, a function of the temperature and the wave-length. The determination of this function is a problem of the highest importance; and though difficulties stand in the way of our effecting this by experiment, there is nevertheless a well-grounded hope of

ultimate success, since the form of the function in question is no doubt simple, as is the case with all functions hitherto discovered that do not depend on the properties of individual bodies. Whenever this problem is solved, the full fertility of the law above demonstrated will be apparent; even at present, however, important consequences may be deduced from it.

§ 12. If a body (a platinum wire, for example) be gradually heated up to a certain temperature, it only emits rays consisting of waves longer than those of the visible rays. Beyond that point, waves of the length of the extreme red begin to appear; and as the temperature rises, shorter and shorter waves are added; so that, for every temperature, rays of a corresponding length of wave are originated, while the intensity of the rays of greater wave-length is increased. If the law we have established be applied to this case, it will be seen that the function I , for waves of any given length, must vanish for all temperatures below that answering to the wave-length in question, and that, for temperatures above this, it must increase with the temperature.

Whence, applying the same proposition to other bodies, it follows that all bodies, when their temperature is gradually raised, begin to emit waves of the same length at the same temperature, and therefore become red-hot at the same temperature, emit yellow rays at the same temperature, &c.* The intensity of rays consisting of waves of a given length, which different bodies emit at the same temperature, may, however, be very different, since it is proportional to the power of absorption of the body for waves of that particular length. At the same temperature, accordingly, metal glows more brightly than glass, and glass more brightly than a gas. A body that remains perfectly transparent at the highest temperature never becomes red-hot. In a platinum ring of about 5 millims. diameter, I placed a small portion of phosphate of soda, and heated it in the dull flame of Bunsen's lamp. The salt melted, formed a fluid lens, and remained perfectly transparent; it, however, emitted no light, while the platinum ring, with which it was in contact, glowed brilliantly.

§ 13. For the same temperature the magnitude I is a continuous function of the wave-length, except for such values of the latter as render I evanescent. The truth of this assertion may be concluded from the continuity of the spectrum of a red-hot platinum-wire, provided it be admitted that the power of absorption of such a body is a continuous function of the length of the waves of the incident rays. It may also be affirmed, with the highest degree of probability, that while the temperature remains

* Draper, *Phil. Mag.* vol. xxx. p. 345; *Berl. Ber.* 1847.

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constant the function I can have no strongly marked maxima and minima for waves of different lengths. Hence it follows that if the spectrum of a red-hot body presents discontinuities or strongly marked maxima or minima, the power of absorption of that body, regarded as a function of the length of the waves, must present similar discontinuities or strongly marked maxima and minima. Spectra with strongly marked maxima may be obtained by placing various salts in the flame of a Bunsen's lamp. Chloride of lithium affords interesting results in this respect. If a bead of this salt be melted in a platinum ring and placed in the mantle of the gas-flame, the spectrum of the flame (when unaffected by the presence of other salts and not too brilliant) is a single bright red line of light, formed of waves whose length is about the arithmetic mean of the lengths of the waves corresponding to the lines B and C of Fraunhofer. For waves of this length the radiating power of the flame is very considerable, while for waves of lengths corresponding to the other visible colours it is imperceptible. Accordingly, the power of absorption of the lithium-flame must be great for waves of this length, but very small for those constituting the other visible rays. If, therefore, a continuous spectrum be formed by suitable means and a lithium-flame be placed between the source of light and the slit of the apparatus, the spectrum is only affected in the place of the lithium line, its brightness being increased in that part by the radiation of the flame, while on the other hand it is diminished by its power of absorption for waves of that particular length. Suppose the absorptive power to be $\frac{1}{4}$. This would be the case, according to the law we have demonstrated, if the brightness of the line which constitutes the spectrum of the lithium-flame were $\frac{1}{4}$ th of that of the corresponding line of the spectrum produced by a black body of the same temperature. The lithium-flame would then be without effect on the spectrum produced by any other source of light, provided the intensity of its own spectrum were $\frac{1}{4}$ th of that of the corresponding line of the spectrum produced in its absence. If the source of light were proportionately brighter than this, the joint effect of it and the lithium-flame would be to produce a comparatively dull line on a bright ground; and conversely, if the source of light were proportionately duller, a bright line on a dull ground would become visible. In the first case the apparent dullness of the line would be greater in proportion to the brightness of the radiating body behind the lithium-flame; for in proportion as the light of the former was increased, so would that of the latter become less observable. For the particular value of the absorptive power mentioned, the brightness of the lithium line can, however, never be less than $\frac{3}{4}$ ths of that of the surrounding parts of the spectrum.

But this limit may be decreased by increasing the thickness of the lithium-flame, and its consequent absorptive power.

A small bead of chloride of lithium, placed in the flame of a Bunsen's lamp, imparts to the latter so considerable an absorptive power for waves of the particular length mentioned, that if the rays of the sun be suffered to fall on the slit of the apparatus that forms the spectrum through such a flame, the corresponding part of the spectrum appears like a fine black line.

The spectra produced when other salts are placed in the flame are for the most part less simple than the lithium spectrum, and seldom exhibit such brilliant lines. *All* of them, however, are capable of being *reversed* by similar means. If flames of sufficient thickness be employed and light of suitable intensity be passed through them, the bright lines of the spectra may all be converted into lines of shade. The only exception would be in the case of a flame the light of which was partly produced by some immediate chemical action, or in case of a fluorescent flame. Experiment must decide whether such flames exist.

If the source of light employed is an incandescent body, the intensity of the light it emits depends on its temperature,—the intensity, for the same temperature, being greatest when the body is perfectly black. If this condition be fulfilled in the case of two sources of light, and if their temperature be the same, the spectrum of the one will be unaffected by the interposition of the other. The more remote source of light can therefore only reverse the spectrum of the other when it possesses a higher temperature, and the reversed spectrum will be more distinct the greater the excess of the temperature of the former source of light over that of the latter.

Besides the spectrum of the lithium-flame, I have succeeded in reversing that of the common salt-flame. This spectrum consists, as is well known, of two very brilliant yellow lines close together, the wave-length of which corresponds to Fraunhofer's double line D. If the rays of a Drummond light be passed through a salt-flame of not too high a temperature, the bright lines of the salt-spectrum become dark, and occupy the place of Fraunhofer's lines D, presenting in every respect the same appearance as those lines*.

§ 14. The wave-lengths which correspond to maxima of the radiating and absorbing powers are, as will be fully explained in another place, altogether independent of the temperature; and

* More recently Prof. Bunsen and I have likewise reversed the brighter lines of the spectra of potassium, calcium, strontium, and barium, by exploding before the slit of the spectral instrument mixtures of milk-sugar and chlorates of the respective metals during the passage of the sun's rays.—May 9, 1860.

moreover, in the case of salts which produce flames having such maxima, it is the *metal* that determines the nature of the spectrum. Imagine a body of very high temperature, in whose spectrum the double line D does not appear, surrounded by a gaseous atmosphere of somewhat lower temperature. If sodium be present in the latter, the spectrum of the whole system so constituted will contain the double line D. From the occurrence of these lines, the presence of sodium in the atmosphere may therefore be concluded. Now the sun is undoubtedly a body of this description*; and therefore, from the occurrence of the lines D in the solar spectrum, the presence of sodium in the sun's atmosphere may be concluded.

An objection may perhaps be urged against the justice of this conclusion. "The cause of the line D is," it may be said, "to be sought for in the atmosphere of the earth." This objection may, however, be disposed of on the following grounds:—

(1) The necessary quantity of sodium in the gaseous form can hardly be present in our atmosphere, and the gaseous form is necessary to produce the effect in question.

(2) If the line D depended on our atmosphere, it would become more strongly marked when the sun approached the horizon. I have, however, never observed any such change in the distinctness of these lines, though in the case of some of the neighbouring lines, such changes are very conspicuous.

(3) If the line D were not caused by the physical constitution of the sun itself, it would exist in the spectra of all the fixed stars of sufficient brightness; but according to Fraunhofer and Brewster, it is wanting in the spectra of some of the fixed stars though present in others.

The precise coincidence of the sodium lines with the D lines of Fraunhofer may be most satisfactorily proved by suffering the sun's rays to fall on the slit of the apparatus through a sodium-flame. The effect of the flame is exhibited in the increased distinctness, darkness, and breadth of the lines D. At the first glance it may appear somewhat strange that the sodium in a small flame should perceptibly increase the effect of the sodium present in the immense mass of the sun's atmosphere. Our surprise at this will, however, be diminished when we consider that the brightness of the lines D of the solar spectrum is determined by the temperature of the solar atmosphere, and especially of its outer portion, and that the temperature of this is certainly much greater than that of a gas-lamp. If a sodium-flame be imagined whose thickness may be regarded as infinite with respect to its power of absorbing the rays that correspond to the

* Whether the central mass of the sun, from which the light principally proceeds, is solid, liquid, or gaseous, may, as far as we are here concerned, be regarded as an open question.

lines D, and if rays from some other source of light be supposed to pass through this flame, and then to be separated into a spectrum, the brightness of the part of the spectrum corresponding to the lines D depends on the radiation of the flame alone. If, then, another sodium-flame of the same temperature be interposed, the spectrum remains unaltered; but if the flame interposed be of lower temperature, the lines must become duller. The effect on the solar spectrum of a gas-flame containing sodium is in this way accounted for, if it be admitted that the temperature of such a flame is lower than that of the outermost envelope of the sun's atmosphere; and this must certainly be the case, since the external portion of the sun's atmosphere cannot have a lower temperature than that of the focus of a powerful concave mirror directed towards the sun.

What has been stated concerning sodium is equally true of every other substance which, when placed in a flame of any sort, produces bright lines in its spectrum. If these lines coincide with the dark lines of the solar spectrum, the presence in the sun's atmosphere of the substances which produce them must be concluded, provided always that the lines in question cannot have their origin in the atmosphere of the earth. In this way means are afforded of determining the chemical constitution of the sun's atmosphere; and the same method even promises some information concerning the constitution of the brighter fixed stars*.

§ 15. From the proposition demonstrated in the first part of this essay, it follows that a body that absorbs more rays polarized in one plane than in another, must emit proportionately more rays of the first description than of the latter. Whence, as is known to be the case, a red-hot opaque body with a smooth surface must emit rays in directions oblique to this surface partly polarized perpendicularly to the plane passing through the ray and normal to the surface; for of the incident rays polarized perpendicularly to the plane of incidence, the body reflects less and absorbs more than of the rays whose plane of polarization is the plane of incidence. By means of this principle the state of polarization of the emitted rays can easily be determined when the law of reflexion of the incident rays is known.

A tourmaline plate split perpendicularly to its optic axis absorbs at ordinary temperatures more of the perpendicularly

* In two communications laid before the Berlin Academy of Sciences on the 27th of October and the 15th of December, 1859, some statements are to be found concerning the physical constitution of the sun's atmosphere which are not introduced here. In the second of those communications also the proposition that forms the principal subject of this essay is proved in another way, but with less generality.

incident rays whose plane of polarization is parallel to the axis of the plate, than of those whose plane of polarization is perpendicular to that axis. If it be granted that the tourmaline retains this property when red-hot, then rays emitted in a direction perpendicular to its surface must be partly polarized in a plane passing through the optic axis, and therefore perpendicular to what is called the plane of polarization of the tourmaline. I have experimentally tested this striking deduction from the law here demonstrated, and have confirmed its truth. The tourmaline plates employed bore a considerable heat in a Bunsen's lamp for some time without suffering any permanent alteration, except that they appeared on cooling to have become a little cloudy at the edges. They retained the property of transmitting polarized light even when red-hot, though to a considerably less degree than at a lower temperature. This appeared on observing through a doubly-refracting prism, a red-hot platinum wire placed behind a tourmaline plate. The two images of the wire so produced were of unequal intensity, though the difference between them was much less than when observed through a plate of the ordinary temperature. To the doubly-refracting prism was then given that position in which the difference of the intensities of the two images was a maximum; if now it was the *upper* image of the wire that was the brightest, then on removing the wire and observing the plate alone, it was found that the *upper* image of the plate was unmistakeably though not strikingly *duller* than the other. The two images appeared exactly like two equal red-hot bodies, of which the upper possessed a lower temperature than the other.

§ 16. Place must be found for one more deduction from the law here established. If a space be entirely surrounded by bodies of the same temperature, so that no rays can penetrate through them, every pencil in the interior of the space must be so constituted, in regard to its quality and intensity, as if it had proceeded from a perfectly black body of the same temperature, and must therefore be independent of the form and nature of the bodies, being determined by the temperature alone. The truth of this is obvious when we reflect that a pencil having the same position but the opposite direction to that chosen, is completely absorbed by the successive reflexions it undergoes from body to body. In the interior therefore of an opaque red-hot body of any temperature, the illumination is always the same, whatever be the constitution of the body in other respects.

It may be observed, by the way, that the proposition demonstrated in this section does not cease to hold good even if some of the bodies are fluorescent. A fluorescent body may be defined as one whose radiating power depends on the rays incident on it

for the time being. The equation $\frac{E}{A}=e$ cannot generally be true of such a body; but it is true if the body is enclosed in a black covering of the same temperature as itself, since the same considerations that led to the equation in question on the hypothesis that the body C was not fluorescent, avail in this case even if the body C be supposed to be fluorescent. To be convinced of this, it is only necessary to consider that if the magnitude E could have two different values in the two arrangements of the system represented in fig. 3, the difference of these values could only be an infinitely small quantity.

Heidelberg, January 1860.

Postscript.*

1. Since the appearance of the above paper in Poggendorff's *Annalen*, I have received information of a prior communication closely related to mine. The communication in question is by Mr. Balfour Stewart, and appeared in the Transactions of the Royal Society of Edinburgh, vol. xxii. 1858. Mr. Stewart has made the interesting observation that a plate of rock-salt is much less diathermic for rays emitted by a mass of the same substance heated to 100° C., than for rays emitted by any other black body of the same temperature. From this circumstance he draws certain conclusions, and is led to a result similar to that which I have established concerning the connexion between the powers of absorption and emission. The principle enunciated by Mr. Stewart is, however, less distinctly expressed, less general, and not altogether so strictly proved as mine. It is as follows:—"The absorption of a plate equals its radiation, and that for every description of heat."

2. The fact that the bright lines of the spectra of sodium- and lithium-flames may be reversed, was first published by me in a communication to the Berlin Academy, October 27, 1859. This communication is noticed by M. Verdet in the February Number of the *Ann. de Chim. et de Phys.* of the following year, and is translated by Prof. Stokes in the March Number of the Philosophical Magazine. The latter gentleman calls attention to a similar observation made by M. Léon Foucault eleven years ago, and which was unknown to me, as it seems to have been to most physicists. This observation was to the effect that the electric arch between charcoal points behaves, with respect to the emission and absorption of rays of refrangibility answering to Fraunhofer's line D, precisely as the sodium-flame does according to my experiments. The communication made on this sub-

* Communicated by the Author.

ject by M. Foucault to the Soc. Philom. in 1849 is reproduced by M. Verdet, from the *Journal de l'Institut*, in the April Number of the *Ann. de Chim. et de Phys.*

M. Foucault's observation appears to be regarded as essentially the same as mine; and for this reason I take the liberty of drawing attention to the difference between the two. The observation of M. Foucault relates to the electric arch between charcoal points, a phenomenon attended by circumstances which are in many respects extremely enigmatical. My observation relates to ordinary flames into which vapours of certain chemical substances have been introduced. By the aid of my observation, the other may be accounted for on the ground of the presence of sodium in the charcoal, and indeed might even have been foreseen. M. Foucault's observation does not afford any explanation of mine, and could not have led to its anticipation. My observation leads necessarily to the law which I have announced with reference to the relation between the powers of absorption and emission; it explains the existence of Fraunhofer's lines, and leads the way to the chemical analysis of the atmosphere of the sun and the fixed stars. All this M. Foucault's observation did not and could not accomplish, since it related to a too complicated phenomenon, and since there was no means of determining how much of the result was due to electricity, and how much to the presence of sodium. If I had been earlier acquainted with this observation, I should not have neglected to introduce some notice of it into my communication, but I should nevertheless have considered myself justified in representing my observation as essentially new.

3. Since the above communication was printed in Poggen-dorff's *Annalen*, I have learned in the course of a written correspondence with Professor Thomson, that the idea was some years ago thrown out, if not published, that it might be possible, by comparing the spectra of various chemical flames with that of the sun and fixed stars, in the manner I have described, to become acquainted with the chemical constitution of the latter bodies (an idea now demonstrated to be correct by the observations and theoretical considerations above set forth). Prof. Thomson writes:—

“Professor Stokes mentioned to me at Cambridge some time ago, probably about ten years, that Professor Miller had made an experiment testing to a very high degree of accuracy the agreement of the double dark line D of the solar spectrum with the double bright line constituting the spectrum of the spirit-lamp burning with salt. I remarked that there must be some physical connexion between two agencies presenting so marked a characteristic in common. He assented, and said he believed

a mechanical explanation of the cause was to be had on some such principles as the following:—Vapour of sodium must possess by its molecular structure a tendency to vibrate in the periods corresponding to the degrees of refrangibility of the double line D. Hence the presence of sodium in a source of light must tend to originate light of that quality. On the other hand, vapour of sodium in an atmosphere round a source, must have a great tendency to retain in itself, *i. e.* to absorb and to have its temperature raised by light from the source, of the precise quality in question. In the atmosphere around the sun, therefore, there must be present vapour of sodium, which, according to the mechanical explanation thus suggested, being particularly opaque for light of that quality, prevents such of it as is emitted from the sun from penetrating to any considerable distance through the surrounding atmosphere. The test of this theory must be had in ascertaining whether or not vapour of sodium has the special absorbing power anticipated. I have the impression that some Frenchman did make this out by experiment, but I can find no reference on the point.

“I am not sure whether Professor Stokes’s suggestion of a mechanical theory has ever appeared in print. I have given it in my lectures regularly for many years, always pointing out along with it that solar and stellar chemistry were to be studied by investigating terrestrial substances giving bright lines in the spectra of artificial flames corresponding to the dark lines of the solar and stellar spectra.”

II. *Illustrations of the Dynamical Theory of Gases.* By J. C. MAXWELL, M.A., Professor of Natural Philosophy in Marischal College and University of Aberdeen.

[Concluded from vol. xix. p. 32.]

PART II. *On the Process of Diffusion of two or more kinds of moving particles among one another.*

WE have shown, in the first part of this paper, that the motions of a system of many small elastic particles are of two kinds: one, a general motion of translation of the whole system, which may be called the motion in mass; and the other a motion of agitation, or molecular motion, in virtue of which velocities in all directions are distributed among the particles according to a certain law. In the cases we are considering, the collisions are so frequent that the law of distribution of the molecular velocities, if disturbed in any way, will be re-established in an inappreciably short time; so that the motion will always con-

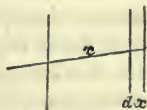
sist of this definite motion of agitation, combined with the general motion of translation.

When two gases are in communication, streams of the two gases might run freely in opposite directions, if it were not for the collisions which take place between the particles. The rate at which they actually interpenetrate each other must be investigated. The diffusion is due partly to the spreading of the particles by the molecular agitation, and partly to the actual motion of the two opposite currents in mass, produced by the pressure behind, and resisted by the collisions of the opposite stream. When the densities are equal, the diffusions due to these two causes respectively are as 2 to 3.

Prop. XIV. *In a system of particles whose density, velocity, &c. are functions of x , to find the quantity of matter transferred across the plane of yz , due to the motion of agitation alone.*

If the number of particles, their velocity, or their length of path is greater on one side of this plane than on the other, then more particles will cross the plane in one direction than in the other; and there will be a transference of matter across the plane, the amount of which may be calculated.

Let there be taken a stratum whose thickness is dx , and area unity, at a distance x from the origin. The number of collisions taking place in this stratum in unit of time will be



$$N \frac{v}{l} dx.$$

The proportion of these which reach a distance between nl and $(n + dn)l$ before they strike another particle is

$$e^{-n} dn.$$

The proportion of these which pass through the plane yz is

$$\frac{nl + x}{2nl} \text{ when } x \text{ is between } -nl \text{ and } 0,$$

and
$$-\frac{nl - x}{2nl} \text{ when } x \text{ is between } 0 \text{ and } +nl;$$

the sign being negative in the latter case, because the particles cross the plane in the negative direction. The mass of each particle is M ; so that the quantity of matter which is projected from the stratum dx , crosses the plane yz in a positive direction, and strikes other particles at distances between nl and $(n + dn)l$ is

$$\frac{MNv(x \mp nl)}{2nl^2} dx e^{-n} dn, \dots \dots \dots (26)$$

where x must be between $\pm nl$, and the upper or lower sign is to be taken according as x is positive or negative.

In integrating this expression, we must remember that N , v , and l are functions of x , not vanishing with x , and of which the variations are very small between the limits $x = -nl$ and $x = +nl$.

As we may have occasion to perform similar integrations, we may state here, to save trouble, that if U and r are functions of x not vanishing with x , whose variations are very small between the limits $x = +r$ and $x = -r$,

$$\int_{-r}^{+r} \pm Ux^m dx = \frac{2}{m+2} \frac{d}{dx} (Ur^{m+2}). \quad (27)$$

When m is an odd number, the upper sign only is to be considered; when m is even or zero, the upper sign is to be taken with positive values of x , and the lower with negative values. Applying this to the case before us,

$$\begin{aligned} \int_{-nl}^{+nl} \frac{MNvx dx}{2nl^2} &= \frac{1}{2} \frac{d}{dx} (MNvn^2l), \\ \int_{-nl}^{+nl} \mp \frac{MNv}{2l} dx &= -\frac{1}{2} \frac{d}{dx} (MNvn^2l). \end{aligned}$$

We have now to integrate

$$\int_0^\infty -\frac{1}{6} \frac{d}{dx} (MNvl) n^2 e^{-n} dn,$$

n being taken from 0 to ∞ . We thus find for the quantity of matter transferred across unit of area by the motion of agitation in unit of time,

$$q = -\frac{1}{2} \frac{d}{dx} (\rho vl), \quad (28)$$

where $\rho = MN$ is the density, v the mean velocity of agitation, and l the mean length of path.

Prop. XV. The quantity transferred, in consequence of a mean motion of translation V , would obviously be

$$Q = V\rho. \quad (29)$$

Prop. XVI. *To find the resultant dynamical effect of all the collisions which take place in a given stratum.*

Suppose the density and velocity of the particles to be functions of x , then more particles will be thrown into the given stratum from that side on which the density is greatest; and those particles which have greatest velocity will have the greatest effect, so that the stratum will not be generally in equilibrium, and the dynamical measure of the force exerted on the stratum will be the resultant momentum of all the particles which lodge in it during unit of time. We shall first take the case in which

there is no mean motion of translation, and then consider the effect of such motion separately.

Let a stratum whose thickness is α (a small quantity compared with l), and area unity, be taken at the origin, perpendicular to the axis of x ; and let another stratum, of thickness dx , and area unity, be taken at a distance x from the first.



If M_1 be the mass of a particle, N the number in unit of volume, v the velocity of agitation, l the mean length of path, then the number of collisions which take place in the stratum dx is

$$= N \frac{v}{l} dx.$$

The proportion of these which reach a distance between nl and $(n+dn)l$ is

$$e^{-n} dn.$$

The proportion of these which have the extremities of their paths in the stratum α is

$$\frac{\alpha}{2nl}.$$

The velocity of these particles, resolved in the direction of x , is

$$= \frac{vx}{nl},$$

and the mass is M ; so that multiplying all these terms together, we get

$$\frac{NMv^2 \alpha x}{2n^2 l^3} e^{-n} dx dn \quad . \quad . \quad . \quad . \quad . \quad (30)$$

for the momentum of the particles fulfilling the above conditions.

To get the whole momentum, we must first integrate with respect to x from $x = -nl$ to $x = +nl$, remembering that l may be a function of x , and is a very small quantity. The result is

$$\frac{d}{dx} \left(\frac{NMv^2}{3} \right) \alpha n e^{-n} dn.$$

Integrating with respect to n from $n=0$ to $n=\infty$, the result is

$$-\alpha \frac{d}{dx} \left(\frac{NMv^2}{3} \right) = \alpha X \rho \quad . \quad . \quad . \quad . \quad (31)$$

as the whole resultant force on the stratum α arising from these collisions. Now $\frac{NMv^2}{3} = p$ by Prop. XII., and therefore we

The effect of these collisions is therefore to produce a force

$$-\frac{dp_1}{dx} \left(A\rho_1 l_1 + B\rho_2 l_1 \frac{M_1}{M_1 + M_2} \right)$$

on particles of the first system, and

$$-\frac{dp_1}{dx} B\rho_2 l_1 \frac{M_2}{M_1 + M_2}$$

on particles of the second system.

The effect of the collisions of those particles of the second system which strike into the stratum, is to produce a force

$$-\frac{dp_2}{dx} C\rho_1 l_2 \frac{M_1}{M_1 + M_2}$$

on the first system, and

$$-\frac{dp_2}{dx} \left(C\rho_1 l_2 \frac{M_2}{M_1 + M_2} + D\rho_2 l_2 \right)$$

on the second.

The whole effect of these collisions is therefore to produce a resultant force

$$-\frac{dp_1}{dx} \left(A\rho_1 l_1 + B\rho_2 l_1 \frac{M_1}{M_1 + M_2} \right) - \frac{dp_2}{dx} C\rho_1 l_2 \frac{M_1}{M_1 + M_2} + \&c. \quad (34)$$

on the first system,

$$-\frac{dp_1}{dx} B\rho_2 l_1 \frac{M_2}{M_1 + M_2} - \frac{dp_2}{dx} \left(C\rho_1 l_2 \frac{M_2}{M_1 + M_2} + D\rho_2 l_2 \right) + \&c. \quad (35)$$

on the second, and so on.

Prop. XVIII. *To find the mechanical effect of a difference in the mean velocity of translation of two systems of moving particles.*

Let V_1, V_2 be the mean velocities of translation of the two systems respectively, then $\frac{M_1 M_2}{M_1 + M_2} (V_1 - V_2)$ is the mean momentum lost by a particle of the first, and gained by a particle of the second at collision. The number of such collisions in unit of volume is

$$N_1 B\rho_2 v_1, \text{ or } N_2 C\rho_1 v_2;$$

therefore the whole effect of the collisions is to produce a force

$$= -N_1 B\rho_2 v_1 \frac{M_1 M_2}{M_1 + M_2} (V_1 - V_2) \quad . \quad . \quad . \quad (36)$$

on the first system, and an equal and opposite force

$$= +N_2 C\rho_1 v_2 \frac{M_1 M_2}{M_1 + M_2} (V_1 - V_2) \quad . \quad . \quad . \quad (37)$$

on unit of volume of the second system.

Prop. XIX. To find the law of diffusion in the case of two gases diffusing into each other through a plug made of a porous material, as in the case of the experiments of Graham.

The pressure on each side of the plug being equal, it was found by Graham that the quantities of the gases which passed in opposite directions through the plug in the same time were directly as the square roots of their specific gravities.

We may suppose the action of the porous material to be similar to that of a number of particles fixed in space, and obstructing the motion of the particles of the moving systems. If L_1 is the mean distance a particle of the first kind would have to go before striking a fixed particle, and L_2 the distance for a particle of the second kind, then the mean paths of particles of each kind will be given by the equations

$$\frac{1}{l_1} = A\rho_1 + B\rho_2 + \frac{1}{L_1}, \quad \frac{1}{l_2} = C\rho_1 + D\rho_2 + \frac{1}{L_2}. \quad (38)$$

The mechanical effect upon the plug of the pressures of the gases on each side, and of the percolation of the gases through it, may be found by Props. XVII. and XVIII. to be

$$\frac{M_1 N_1 v_1 V_1}{L_1} + \frac{M_2 N_2 v_2 V_2}{L_2} - \frac{dp_1}{dx} \frac{l_1}{L_1} - \frac{dp_2}{dx} \frac{l_2}{L_2} = 0; \quad (39)$$

and this must be zero, if the pressures are equal on each side of the plug. Now if Q_1, Q_2 be the quantities transferred through the plug by the mean motion of translation, $Q_1 = \rho_1 V_1 = M_1 N_1 V_1$; and since by Graham's law

$$\frac{Q_1}{Q_2} = - \sqrt{\frac{M_1}{M_2}} = - \frac{v_2}{v_1},$$

we shall have

$$M_1 N_1 v_1 V_1 = - M_2 N_2 v_2 V_2 = U \text{ suppose;}$$

and since the pressures on the two sides are equal, $\frac{dp_2}{dx} = - \frac{dp_1}{dx}$,

and the only way in which the equation of equilibrium of the plug can generally subsist is when $L_1 = L_2$ and $l_1 = l_2$. This implies that $A = C$ and $B = D$. Now we know that $v_1^3 B = v_2^3 C$.

Let $K = 3 \frac{A}{v_1^3}$, then we shall have

$$A = C = \frac{1}{3} K v_1^3, \quad B = D = \frac{1}{3} K v_2^3. \quad (40)$$

and

$$\frac{1}{l_1} = \frac{1}{l_2} = K(v_1 p_1 + v_2 p_2) + \frac{1}{L}. \quad (41)$$

The diffusion is due partly to the motion of translation, and partly to that of agitation. Let us find the part due to the motion of translation.

The equation of motion of one of the gases through the plug is found by adding the forces due to pressures to those due to resistances, and equating these to the moving force, which in the case of slow motions may be neglected altogether. The result for the first is

$$\frac{dp_1}{dx} \left(A\rho_1 l_1 + B\rho_2 l_1 \frac{M_1}{M_1 + M_2} \right) + \frac{dp_2}{dx} C\rho_1 l_2 \frac{M_1}{M_1 + M_2} + N_1 B\rho_2 v_1 \frac{M_1 M_2}{M_1 + M_2} (V_1 - V_2) + \frac{\rho_1 v_1 V_1}{L} = 0. \quad (42)$$

Making use of the simplifications we have just discovered, this becomes

$$\frac{dp}{dx} \frac{KL}{v_1^3 + v_2^3} (v_1^3 p_1 + v_2^3 p_2) + K \frac{v_1 v_2}{v_1^2 + v_2^2} (p_1 v_2 + p_2 v_1) U + \frac{1}{l} U, \quad (43)$$

whence

$$U = - \frac{dp}{dx} \frac{KL(v_1^3 p_1 + v_2^3 p_2)}{Kv_1 v_2 (p_1 v_2 + p_2 v_1) + \frac{v_1^2 + v_2^2}{L}}; \quad (44)$$

whence the rate of diffusion due to the motion of translation may be found; for

$$Q_1 = \frac{U}{v_1}, \text{ and } Q_2 = - \frac{U}{v_2}. \quad (45)$$

To find the diffusion due to the motion of agitation, we must find the value of q_1 .

$$\begin{aligned} q_1 &= -\frac{1}{3} \frac{d}{dx} (\rho_1 v_1 l_1), \\ &= -\frac{L}{v_1} \frac{d}{dx} \frac{p_1}{1 + KL(v_1 p_1 + v_2 p_2)}, \\ q_1 &= -\frac{l^2}{v_1 L} \frac{dp}{dx} (1 + KLv_2 (p_1 + p_2)). \quad (46) \end{aligned}$$

Similarly,

$$q_2 = + \frac{l^2}{v_2 L} \frac{dp}{dx} (1 + KLv_1 (p_1 + p_2)). \quad (47)$$

The whole diffusions are $Q_1 + q_1$ and $Q_2 + q_2$. The values of q_1 and q_2 have a term not following Graham's law of the square roots of the specific gravities, but following the law of equal volumes. The closer the material of the plug, the less will this term affect the result.

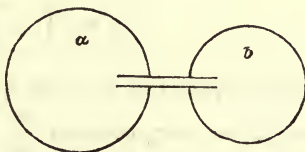
Our assumptions that the porous plug acts like a system of fixed particles, and that Graham's law is fulfilled more accurately the more compact the material of the plug, are scarcely sufficiently well verified for the foundation of a theory of gases; and

even if we admit the original assumption that they are systems of moving elastic particles, we have not very good evidence as yet for the relation among the quantities A, B, C, and D.

Prop. XX. *To find the rate of diffusion between two vessels connected by a tube.*

When diffusion takes place through a large opening, such as a tube connecting two vessels, the question is simplified by the absence of the porous diffusion plug; and since the pressure is constant throughout the apparatus, the volumes of the two gases passing opposite ways through the tube at the same time must be equal. Now the quantity of gas which passes through the tube is due partly to the motion of agitation as in Prop. XIV., and partly to the mean motion of translation as in Prop. XV.

Let us suppose the volumes of the two vessels to be a and b , and the length of the tube between them c , and its transverse section s . Let a be filled with the first gas, and b with the second at the commencement of



the experiment, and let the pressure throughout the apparatus be P .

Let a volume y of the first gas pass from a to b , and a volume y' of the second pass from b to a ; then if p_1 and p_2 represent the pressures in a due to the first and second kinds of gas, and p'_1 and p'_2 the same in the vessel b ,

$$p_1 = \frac{a-y}{a} P, \quad p_2 = \frac{y'}{a} P, \quad p'_1 = \frac{y}{b} P, \quad p'_2 = \frac{b-y'}{b} P. \quad (48)$$

Since there is still equilibrium,

$$p_1 + p_2 = p'_1 + p'_2,$$

which gives

$$y = y' \text{ and } p_1 + p_2 = P = p'_1 + p'_2. \quad (49)$$

The rate of diffusion will be $+\frac{dy}{dt}$ for the one gas, and $-\frac{dy}{dt}$ for the other, measured in volume of gas at pressure P .

Now the rate of diffusion of the first gas will be

$$\frac{dy}{dt} = s \frac{k_1 q_1 + p_1 V_1}{P} = s \frac{-\frac{1}{3} v_1 \frac{d}{dx} (p_1 l_1) + p_1 V_1}{P}; \quad (50)$$

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and that of the second,

$$-\frac{dy}{dt} = s \frac{-\frac{1}{3}v_2 \frac{d}{dx}(p_2 l_2) + p_2 V_2}{P}. \quad (51)$$

We have also the equation, derived from Props. XVI. and XVII.,

$$\begin{aligned} \frac{dp_1}{dx} (A\rho_1 l_1 (M_1 + M_2) + B\rho_2 l_1 M_1 - C\rho_1 l_2 M_1) \\ + B\rho_1 \rho_2 v_1 M_2 (V_1 - V_2) = 0. \end{aligned} \quad (52)$$

From these three equations we can eliminate V_1 and V_2 , and find $\frac{dy}{dt}$ in terms of p and $\frac{dp}{dx}$, so that we may write

$$\frac{dy}{dt} = f\left(p_1, \frac{dp_1}{dx}\right). \quad (53)$$

Since the capacity of the tube is small compared with that of the vessels, we may consider $\frac{dy}{dt}$ constant through the whole length of the tube. We may then solve the differential equation in p and x ; and then making $p=p_1$ when $x=0$, and $p=p'_1$ when $x=c$, and substituting for p_1 and p'_1 their values in terms of y , we shall have a differential equation in y and t , which being solved, will give the amount of gas diffused in a given time.

The solution of these equations would be difficult unless we assume relations among the quantities A, B, C, D , which are not yet sufficiently established in the case of gases of different density. Let us suppose that in a particular case the two gases have the same density, and that the four quantities A, B, C, D are all equal.

The volume diffused, owing to the motion of agitation of the particles, is then

$$-\frac{1}{3} \frac{s}{P} \frac{dp}{dx} vl,$$

and that due to the motion of translation, or the interpenetration of the two gases in opposite streams, is

$$-\frac{s}{P} \frac{dp}{dx} \frac{kl}{v}.$$

The values of v are distributed according to the law of Prop. IV., so that the mean value of v is $\frac{2\alpha}{\sqrt{\pi}}$, and that of $\frac{1}{v}$ is $\frac{2}{\sqrt{\pi}\alpha}$, that of k being $\frac{1}{2}\alpha^2$. The diffusions due to these two causes are

therefore in the ratio of 2 to 3, and their sum is

$$\frac{dy}{dt} = -\frac{4}{3} \sqrt{\frac{2k}{\pi}} \frac{sl}{P} \frac{dp}{dx} \quad \dots \quad (54)$$

If we suppose $\frac{dy}{dt}$ constant throughout the tube, or, in other words, if we regard the motion as *steady* for a short time, then $\frac{dp}{dx}$ will be constant and equal to $\frac{p'_1 - p_1}{c}$; or substituting from (48),

$$\frac{dy}{dt} = -\frac{4}{3} \sqrt{\frac{2k}{\pi}} \frac{sl}{abc} ((a+b)y - ab), \quad \dots \quad (55)$$

whence

$$y = \frac{ab}{a+b} (1 - e^{-\frac{4}{3} \sqrt{\frac{2k}{\pi}} \frac{sl}{abc} (a+b)t}). \quad \dots \quad (56)$$

By choosing pairs of gases of equal density, and ascertaining the amount of diffusion in a given time, we might determine the value of l in this expression. The diffusion of nitrogen into carbonic oxide or of deutoxide of nitrogen into carbonic acid, would be suitable cases for experiment. The only existing experiment which approximately fulfils the conditions is one by Graham, quoted by Herapath from Brande's Quarterly Journal of Science, vol. xviii. p. 76.

A tube 9 inches long and 0.9 inch diameter, communicated with the atmosphere by a tube 2 inches long and 0.12 inch diameter; 152 parts of olefiant gas being placed in the tube, the quantity remaining after four hours was 99 parts.

In this case there is not much difference of specific gravity between the gases, and we have $a = 9 \times (0.9)^2 \frac{\pi}{4}$ cubic inches, $b = \infty$, $c = 2$ inches, and $s = (0.12)^2 \frac{\pi}{4}$ square inches;

$$l = \sqrt{\frac{\pi}{2k}} \frac{3}{4} \frac{ac}{s} \log_e 10 \cdot \frac{1}{t} \cdot \log_{10} \left(\frac{a}{a-y} \right); \quad \dots \quad (57)$$

$$\therefore l = 0.00000256 \text{ inch} = \frac{1}{389000} \text{ inch}. \quad \dots \quad (58)$$

Prop. XXI. To find the amount of energy which crosses unit of area in unit of time when the velocity of agitation is greater on one side of the area than on the other.

The energy of a single particle is composed of two parts,—the *vis viva* of the centre of gravity, and the *vis viva* of the various motions of rotation round that centre, or, if the particle be capable of internal motions, the *vis viva* of these. We shall suppose that the whole *vis viva* bears a constant proportion to that

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 due to the motion of the centre of gravity, or

$$E = \frac{1}{2} \beta M v^2,$$

where β is a coefficient, the experimental value of which is 1.634. Substituting E for M in Prop. XIV., we get for the transference of energy across unit of area in unit of time,

$$Jgq = -\frac{1}{2} \frac{d}{dx} \left(\frac{1}{2} \beta M v^2 N v l \right),$$

where J is the mechanical equivalent of heat in foot-pounds, and q is the transfer of heat in thermal units.

Now $MN = \rho$, and $l = \frac{1}{A\rho}$, so that $MNl = \frac{1}{A}$;

$$\therefore Jgq = -\frac{1}{2} \frac{\beta v^2}{A} \frac{dv}{dx}. \quad \dots \dots \dots (59)$$

Also, if T is the absolute temperature,

$$\frac{1}{T} \frac{dT}{dx} = \frac{2}{v} \frac{dv}{dx};$$

$$\therefore Jgq = -\frac{5}{4} \beta p l v \frac{1}{T} \frac{dT}{dx}, \quad \dots \dots \dots (60)$$

where p must be measured in dynamical units of force.

Let $J = 772$ foot-pounds, $p = 2116$ pounds to square foot, $l = \frac{1}{400000}$ inch, $v = 1505$ feet per second, $T = 522$ or 62° Fahrenheit; then

$$q = \frac{T' - T}{40000x}, \quad \dots \dots \dots (61)$$

where q is the flow of heat in thermal units per square foot of area; and T' , and T are the temperatures at the two sides of a stratum of air x inches thick.

In Prof. Rankine's work on the Steam-engine, p. 259, values of the *thermal resistance*, or the reciprocal of the *conductivity*, are given for various substances as computed from a Table of conductivities deduced by M. Peclet from experiments by M. Despretz:—

	Resistance.
Gold, Platinum, Silver.	0.0036
Copper	0.0040
Iron	0.0096
Lead	0.0198
Brick	0.3306
Air by our calculation	40000

It appears, therefore, that the resistance of a stratum of air to the conduction of heat is about 10,000,000 times greater than

that of a stratum of copper of equal thickness. It would be almost impossible to establish the value of the conductivity of a gas by direct experiment, as the heat radiated from the sides of the vessel would be far greater than the heat conducted through the air, even if currents could be entirely prevented.

PART III. *On the Collision of Perfectly Elastic Bodies of any Form.*

When two perfectly smooth spheres strike each other, the force which acts between them always passes through their centres of gravity; and therefore their motions of rotation, if they have any, are not affected by the collision, and do not enter into our calculations. But; when the bodies are not spherical, the force of compact will not, in general, be in the line joining their centres of gravity; and therefore the force of impact will depend both on the motion of the centres and the motions of rotation before impact, and it will affect both these motions after impact.

In this way the velocities of the centres and the velocities of rotation will act and react on each other, so that finally there will be some relation established between them; and since the rotations of the particles about their three axes are quantities related to each other in the same way as the three velocities of their centres, the reasoning of Prop. IV. will apply to rotation as well as velocity, and both will be distributed according to the law

$$\frac{dN}{dx} = N \frac{1}{\alpha \sqrt{\pi}} e^{-\frac{x^2}{\alpha^2}}.$$

Also, by Prop. V., if x be the average velocity of one set of particles, and y that of another, then the average value of the sum or difference of the velocities is

$$\sqrt{x^2 + y^2};$$

from which it is easy to see that, if in each individual case

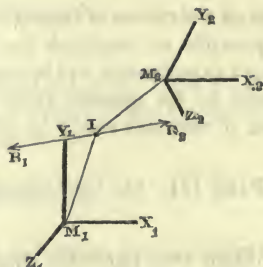
$$u = ax + by + cz,$$

where x, y, z are independent quantities distributed according to the law above stated, then the *average values* of these quantities will be connected by the equation

$$u^2 = a^2 x^2 + b^2 y^2 + c^2 z^2.$$

Prop. XXII. *Two perfectly elastic bodies of any form strike each other: given their motions before impact, and the line of impact, to find their motions after impact.*

Let M_1 and M_2 be the centres of gravity of the two bodies. $M_1 X_1$, $M_1 Y_1$, and $M_1 Z_1$ the principal axes of the first; and $M_2 X_2$, $M_2 Y_2$, and $M_2 Z_2$ those of the second. Let I be the point of impact, and $R_1 I R_2$ the line of impact.



Let the coordinates of I with respect to M_1 be $x_1 y_1 z_1$, and with respect to M_2 let them be $x_2 y_2 z_2$.

Let the direction-cosines of the line of impact $R_1 I R_2$ be

$l_1 m_1 n_1$ with respect to M_1 , and $l_2 m_2 n_2$ with respect to M_2 .

Let M_1 and M_2 be the masses, and $A_1 B_1 C_1$ and $A_2 B_2 C_2$ the moments of inertia of the bodies about their principal axes.

Let the velocities of the centres of gravity, resolved in the direction of the principal axes of each body, be

$U_1 V_1 W_1$ and $U_2 V_2 W_2$ before impact,

and

$U'_1 V'_1 W'_1$ and $U'_2 V'_2 W'_2$ after impact.

Let the angular velocities round the same axes be

$p_1 q_1 r_1$ and $p_2 q_2 r_2$ before impact,

and

$p'_1 q'_1 r'_1$ and $p'_2 q'_2 r'_2$ after impact.

Let R be the impulsive force between the bodies, measured by the momentum it produces in each.

Then, for the velocities of the centres of gravity, we have the following equations:

$$U'_1 = U_1 + \frac{R l_1}{M_1}, \quad U'_2 = U_2 - \frac{R l_2}{M_2}, \quad \dots \quad (62)$$

with two other pairs of equations in V and W . The equations for the angular velocities are

$$p'_1 = p_1 + \frac{R}{A_1} (y_1 n_1 - z_1 m_1), \quad p'_2 = p_2 - \frac{R}{A_2} (y_2 n_2 - z_2 m_2), \quad (63)$$

with two other pairs of equations for q and r .

The condition of perfect elasticity is that the whole *vis viva* shall be the same after impact as before, which gives the equation $M_1(U_1'^2 - U_1^2) + M_2(U_2'^2 - U_2^2) + A_1(p_1'^2 - p_1^2) + A_2(p_2'^2 - p_2^2) + \&c. = 0$. (64)

The terms relating to the axis of x are here given; those relating to y and z may be easily written down.

Substituting the values of these terms, as given by equations

(62) and (63), and dividing by R, we find

$$l_1(U'_1 + U_1) - l_2(U'_2 + U_2) + (y_1n_1 - z_1m_1)(p'_1 + p_1) - (y_2n_2 - z_2m_2)(p'_2 + p_2) + \&c. = 0. \quad (65)$$

Now if v_1 be the velocity of the striking-point of the first body before impact, resolved along the line of impact,

$$v_1 = l_1U_1 + (y_1n_1 - z_1m_1)p_1 + \&c.;$$

and if we put v_2 for the velocity of the other striking-point resolved along the same line, and v'_1 and v'_2 the same quantities after impact, we may write equation (65),

$$v_1 + v'_1 - v_2 - v'_2 = 0, \quad (66)$$

or

$$v_1 - v_2 = v'_2 - v'_1, \quad (67)$$

which shows that the velocity of separation of the striking-points resolved in the line of impact is equal to that of approach.

Substituting the values of the accented quantities in equation (65) by means of equations (63) and (64), and transposing terms in R, we find

$$2\{U_1l_1 - U_2l_2 + p_1(y_1n_1 - z_1m_1) - p_2(y_2n_2 - z_2m_2)\} + \&c. = -R \left\{ \frac{l_1^2}{M_1} + \frac{l_2^2}{M_2} + \frac{(y_1n_1 - z_1m_1)^2}{A_1} + \frac{(y_2n_2 - z_2m_2)^2}{A_2} + \&c. \right\}, \quad (68)$$

the other terms being related to y and z as these are to x . From this equation we may find the value of R; and by substituting this in equations (63), (64), we may obtain the values of all the velocities after impact.

We may, for example, find the value of U'_1 from the equation

$$\left. \begin{aligned} U'_1 \left\{ \frac{l_1^2}{M_1} + \frac{l_2^2}{M_2} + \frac{(y_1n_1 - z_1m_1)^2}{A_1} + \frac{(y_2n_2 - z_2m_2)^2}{A_2} + \&c. \right\} \frac{M_1}{l_1} \\ = U_1 \left\{ -\frac{l_1^2}{M_1} + \frac{l_2^2}{M_2} + \frac{(y_1n_1 - z_1m_1)^2}{A_1} + \frac{(y_2n_2 - z_2m_2)^2}{A_2} + \&c. \right\} \frac{M_1}{l_1} \\ + 2U_2l_2 - 2p_1(y_1n_1 - z_1m_1) + 2p_2(y_2n_2 - z_2m_2) - \&c. \end{aligned} \right\} \quad (69)$$

Prop. XXIII. *To find the relations between the average velocities of translation and rotation after many collisions among many bodies.*

Taking equation (69), which applies to an individual collision, we see that U'_1 is expressed as a linear function of $U_1, U_2, p_1, p_2, \&c.$, all of which are quantities of which the values are distributed among the different particles according to the law of Prop. IV. It follows from Prop. V., that if we square every term of the equation, we shall have a new equation between the *average values* of the different quantities. It is plain that, as soon as the required relations have been established, they will remain the

same after collision, so that we may put $U_1'^2 = U_1^2$ in the equation of averages. The equation between the average values may then be written

$$(M_1 U_1^2 - M_2 U_2^2) \frac{l_2^2}{M_2} + (M_1 U_1^2 - A_1 p_1^2) \frac{(y_1 n_1 - z_1 m_1)^2}{A_1} \\ + (M_1 U_1^2 - A_2 p_2^2) \frac{(y_2 n_2 - z_2 m_2)^2}{A_2} + \&c. = 0.$$

Now since there are collisions in every possible way, so that the values of $l, m, n, \&c.$ and $x, y, z, \&c.$ are infinitely varied, this equation cannot subsist unless

$$M_1 U_1^2 = M_2 U_2^2 = A_1 p_1^2 = A_2 p_2^2 = \&c.$$

The final state, therefore, of any number of systems of moving particles of any form is that in which the average *vis viva* of translation along each of the three axes is the same in all the systems, and equal to the average *vis viva* of rotation about each of the three principal axes of each particle.

Adding the *vires vivæ* with respect to the other axes, we find that the whole *vis viva* of translation is equal to that of rotation in each system of particles, and is also the same for different systems, as was proved in Prop. VI.

This result (which is true, however nearly the bodies approach the spherical form, provided the motion of rotation is at all affected by the collisions) seems decisive against the unqualified acceptance of the hypothesis that gases are such systems of hard elastic particles. For the ascertained fact that γ , the ratio of the specific heat at constant pressure to that at constant volume, is equal to 1.408, requires that the ratio of the whole *vis viva* to the *vis viva* of translation should be

$$\beta = \frac{2}{3(\gamma - 1)} = 1.634;$$

whereas, according to our hypothesis, $\beta = 2$.

We have now followed the mathematical theory of the collisions of hard elastic particles through various cases, in which there seems to be an analogy with the phenomena of gases. We have deduced, as others have done already, the relations of pressure, temperature, and density of a single gas. We have also proved that when two different gases act freely on each other (that is, when at the same temperature), the mass of the single particles of each is inversely proportional to the square of the molecular velocity; and therefore, at equal temperature and pressure, *the number of particles in unit of volume is the same.*

We then offered an explanation of the internal friction of

gases, and deduced from experiments a value of the mean length of path of a particle between successive collisions.

We have applied the theory to the law of diffusion of gases, and, from an experiment on olefiant gas, we have deduced a value of the length of path not very different from that deduced from experiments on friction.

Using this value of the length of path between collisions, we found that the resistance of air to the conduction of heat is 10,000,000 that of copper, a result in accordance with experience.

Finally, by establishing a necessary relation between the motions of translation and rotation of all particles not spherical, we proved that a system of such particles could not possibly satisfy the known relation between the two specific heats of all gases.

III. *On a New Theoretical Determination of the Velocity of Sound.* By the Rev. S. EARNSHAW, M.A., Sheffield.

[Continued from vol. xix. p. 455.]

On the Velocity of the Sound of Thunder.

THERE yet remains to be considered a case of sound-velocity to which the investigations of Newton and the suggestion of Laplace are totally inadequate, which nevertheless is naturally suggested, by what has been done in the preceding articles, as necessary to complete the theory of sound-velocity: I allude to the propagation of the sound of a clap of thunder. The consideration of this case will strengthen the evidence of the soundness of the preceding investigations.

Before it was announced by myself at the Meeting of the British Association at Leeds in 1858, that according to theory violent sounds are propagated more rapidly than gentle sounds, I believe the fact was not suspected by philosophers. I was led to this result by a careful discussion of the integral of the well-known equation of motion of an elastic fluid in a horizontal tube. I was, however, not able to bring forward any instance of the fact having been observed, except a single one, recorded in one of Parry's Voyages to the North. The records of experimentalists agreed in stating, on the contrary, that all sounds travel at the same rate. Since that time the subject has rested. A few weeks ago, however, my attention was recalled to it by the receipt of a memoir printed in the *Bulletins de l'Académie Royale de Belgique*, kindly forwarded to me by its author, Professor Ch. Montigny of Antwerp, which has satisfied me that, in the case of a thunder-clap, sound is sometimes propagated with a velocity far greater than I had ever imagined, and that the problem of the propagation of sound is yet far from having been fully solved.

In the spring of 1851, when I was living in the small village of Conisborough about five miles from Doncaster, there occurred, one Sunday evening about 5 o'clock, a thunder-storm which lasted about half an hour. It was not different from other thunder-storms, except that it was terminated by a flash of lightning of great vividness, which was *instantly* (i. e. without any *appreciable* interval between) followed by an awful crash, that seemed as if by atmospheric concussion alone it would crush the cottages to ruins. Every one in the village, as I found afterwards, had felt at the moment of the crash, as I felt myself, that the electric fluid had certainly fallen somewhere in the village. Indeed it seemed impossible for any one to think otherwise than that his neighbour's cottage had been struck, and his own had had a narrow escape. But, to the surprise of everybody, it turned out that no damage had been done in the village, but that that flash of lightning had killed three sheep, knocked down a cow, and injured the milkmaid who happened to be milking the cow, at a distance of more than a mile from the village. I was at that time under a physician's orders not to think about anything, but I remember thinking that it was very singular the lightning should fall at such a distance, when I should have expected it, from the received theory of sound, to fall in the village. Professor Montigny's memoir records similar, and yet more striking instances, which have come to his knowledge or been observed by himself. In one of them the report of a thunder-clap reached him after an interval of 2 seconds, which according to theory should have occupied more than 15 seconds. As he well remarks, the difference in this instance is too great to be accounted for by the agency of wind, or errors of observation; besides which, his observation was confirmed by an independent observer, the curé of a parish a few miles off, who observed the same flash and estimated the interval that elapsed before the report reached him at 2 seconds, though according to theory it should have been nearly 15 seconds, the two observers and the point struck by the electric fluid being nearly equidistant from one another. Other cases, less striking perhaps, but quite agreeing with this, are recorded in the memoir; and after reading them and remembering my own experience, I cannot but consider this a subject worthy of the attention of observers. And if it shall seem good to them to record their observations in cases where, from some noticeable peculiarity, the point struck by a particular flash can be identified, I strongly suspect it will soon be established as a law of nature, that *the sound of a thunder-clap is propagated with far greater rapidity than ordinary sounds*. We shall presently see what light theory throws upon this point.

9. The velocity of sound has been determined in the last

article by the assumption of the musical type of the sound-wave. The result shows that all audible musical sounds are transmitted with the same velocity: and, as the equation of motion in art. 3 is linear, it follows that every sound which can be considered as a combination of musical sounds, will be transmitted with the same velocity: and as in general any ordinary function of t can be expanded in the form $A' \cos (k' t + a') + A'' \cos (k'' t + a'') + \dots$, it follows that all ordinary sounds are made up of musical sounds and transmitted with the same velocity.

10. From this it will be seen that we may consider the equation

$$D_t^2 x + k^2 x = 0$$

as the definition of the elements of what I have denominated a *gentle* or *ordinary* sound. The convenience of analysis requires that, in treating the differential equation of art. 3, we should be able to express $D_t^2 x_r$ in the form of x_r multiplied into a constant, or to substitute a constant for the symbol D_t^2 ; and hence it is obvious there remains yet to be considered the form

$$D_t^2 x_r - k^2 x_r = 0.$$

In other words, it remains that we should consider the exponential type $x_r = A_r \epsilon^{kt}$, which we shall consider to be the type of elementary *violent* sounds.

By the substitution of this form in the equation of art. 3 we obtain

$$k^2 A_r = mf'(h) \cdot (A_{r-1} - 2A_r + A_{r+1}) + \&c.,$$

which being a linear equation of finite differences, we find for its solution

$$A_r = C\alpha^{2r} + C'\alpha^{-2r}, \dots \dots \dots (1')$$

the quantity α being such as to satisfy the equation

$$k^2 = mf'(h) \cdot (\alpha - \alpha^{-1})^2 + mf'(2h) \cdot (\alpha^2 - \alpha^{-2})^2 + \dots \dots (2')$$

the form of which indicates that, for every value of k , there is a possible value of $\alpha - \alpha^{-1}$, and therefore of α . The case of $\alpha - \alpha^{-1} = 0$ is excluded because it makes $k = 0$, and destroys the previous assumption of the exponential type. I assume therefore the value of $\alpha - \alpha^{-1}$ to be the measure of the degree of violence of the genesis of any proposed wave.

The above leads to the equation

$$x_r = C\epsilon^{kt+2r \log_e \alpha} + C'\epsilon^{kt-2r \log_e \alpha},$$

the two terms of which indicate the possible coexistence, at the same point, of two violent waves travelling in opposite directions. We shall therefore only retain the latter term,

$$x_r = A\epsilon^{kt-2r \log_e \alpha}, \dots \dots \dots (5')$$

as the representative of a progressive wave of the violent type.

From this equation we determine, as before, the velocity of a violent sound to be

$$v = \frac{hk}{2 \log_e \alpha} = \frac{h \sqrt{m}}{2 \log_e \alpha} \cdot \{f'(h) \cdot (\alpha - \alpha^{-1})^2 + f'(2h) \cdot (\alpha^2 - \alpha^{-2})^2 + \dots\}^{\frac{1}{2}}, \quad (7')$$

a result which may be written in the form

$$\frac{v}{V} = \frac{\alpha - \alpha^{-1}}{2 \log_e \alpha} \cdot \left\{ \frac{1 + \frac{1}{2^2} \left(\frac{\alpha + \alpha^{-1}}{2} \right)^2 + \frac{1}{3^2} \left(\frac{\alpha^2 + 1 + \alpha^{-2}}{3} \right)^2 + \dots}{1 + \frac{1}{2^2} + \frac{1}{3^2} + \dots} \right\}^{\frac{1}{2}}. \quad (8)$$

V being the velocity, before determined in art. 8, for all gentle sounds. That the full meaning of this equation may be obvious, it is necessary to remember that

$$\frac{\alpha - \alpha^{-1}}{2 \log_e \alpha} = 1 + \frac{(\log_e \alpha)^2}{1 \cdot 2 \cdot 3} + \frac{(\log_e \alpha)^4}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} + \dots \quad (9)$$

11. It is manifest, from inspection of the last two equations, that when $\alpha=1$ or $\alpha - \alpha^{-1}=0$, then $v=V$. This case therefore connects (or, rather, stands between as the limit of both) the *gentle* and the *violent* sounds. But for every value of α greater than unity, we perceive that v must necessarily be greater than V , which is the limiting velocity of gentle sounds, for both the factors of the right-hand term of equation (8) are greater than unity.

12. In art. 8 we found an expression for the velocity of transmission of ordinary sounds which was independent of the pitch. But equation (8) we perceive involves $\alpha - \alpha^{-1}$, the degree of violence belonging to each wave: and hence it is not true of violent sounds that they all travel with the same velocity. The elements of a compound violent sound will also not keep together unless the value of α be the same in all, or vary continuously from one element to another. In the latter case the sound will, by the unequal motion of its elements, have a tendency to lengthen; and if α do not vary continuously, the sound will soon be heard, not as one clap, but as a series of minor claps, or as a *rattle*.

13. It is seen from observing the forms of equations (8) and (9), that such a value can be found for α as shall make the ratio $\frac{v}{V}$ as large as we please. It would appear therefore that there is no other limit to the velocity with which a violent sound is transmissible through the atmosphere, than that which the possibility of supplying a sufficient degree of force in its genesis may impose.

Hence it is probable that there is no sound which is propagated faster than a thunder-clap, the genesis of which by the electric discharge being extremely violent and almost instantaneous, and accompanied by a large development of heat.

If the theory here advanced be true, the report of fire-arms should travel faster than the human voice; and the crash of thunder faster than the report of a cannon.

It will be admitted, I think, that the expression for the velocity in equation (8) is capable of furnishing any required amount of velocity. The difficulty of putting it to the test of experiment lies in our utter inability to say, in any proposed case, what is the value of α . All that it seems possible to do is to observe v , and thence calculate α by the equation referred to; and then to judge, by the comparison of different observations, whether the calculated values of $\alpha - \alpha^{-1}$ seem to agree with the observed violence of the electric discharges.

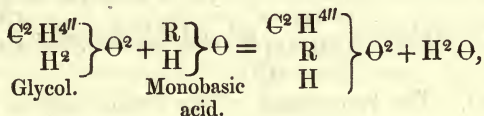
Sheffield, June 11, 1860.

[To be continued.]

IV. *Chemical Notices from Foreign Journals.* By E. ATKINSON, Ph.D., F.C.S., Teacher of Physical Science in Cheltenham College.

[Continued from vol. xix. p. 390.]

LOURENÇO* has prepared several of the compound ethers of glycol. The ethers with a single acid radical are formed according to the reaction



in which R is an acid radical. The preparation is effected by heating equivalent quantities of the substances together in a

sealed tube. In this manner the monoacetate $\left. \begin{array}{c} \text{C}^2 \text{H}^{4''} \\ \text{H} \end{array} \right\} \Theta^2$,

the monobutyrate $\left. \begin{array}{c} \text{C}^2 \text{H}^{4''} \\ \text{C}^4 \text{H}^7 \Theta \end{array} \right\} \Theta^2$, and the monovalerianate $\left. \begin{array}{c} \text{C}^2 \text{H}^{4''} \\ \text{C}^5 \text{H}^9 \Theta \end{array} \right\} \Theta^2$ have been obtained. The monoacetate prepared

by this method corresponded in properties with that obtained by another process†. The monobutyrate and monovalerianate are

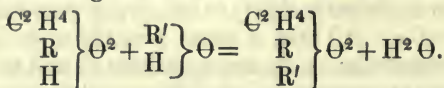
* *Bulletin de la Société Chimique*, p. 90.

† *Phil. Mag.* vol. xvi. p. 433.

colourless, oily liquids, insoluble in water, but readily soluble in alcohol and in ether, and with odours resembling their corresponding acids. They boil respectively at 220° and 240° C.

The ethers with two acid radicals of the same kind are formed by heating glycol with excess of the acid in question. *Divale-rianate of glycol*, $\left. \begin{matrix} \text{C}^2\text{H}^{4''} \\ \text{C}^5\text{H}^9\Theta \end{matrix} \right\} \Theta^2$, is an oily liquid insoluble in water, and boiling at 255°.

The mixed ethers are prepared by treating with another acid the ethers with a single acid radical. Thus:



In this manner Lourenço has prepared acetobutyric acid, already

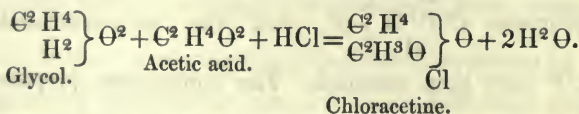
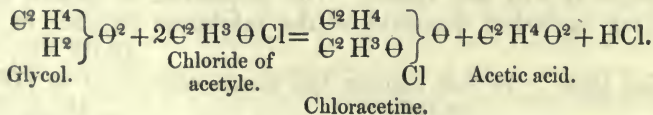
obtained by Simpson*, and *acetovalerianic acid*, $\left. \begin{matrix} \text{C}^2\text{H}^4 \\ \text{C}^2\text{H}^3\Theta \\ \text{C}^5\text{H}^9\Theta \end{matrix} \right\} \Theta^2$.

This latter is a colourless, oily, neutral liquid, soluble in ether and in alcohol, and boiling at 230° C.

These ethers distil without decomposition. When treated with water, they experience a decomposition analogous to that of the ethers of alcohol and glycerine, being resolved into an acid and glycol, or an intermediate compound. Thus neutral acetate of glycol, when treated with water, is resolved into free acid and monoacetate of glycol; and monoacetate of glycol, when treated with water, is decomposed into free acetic acid and glycol.

By the action of chloride of acetylene or butyryle on glycol, the chloroacetine of glycol, $\left. \begin{matrix} \text{C}^2\text{H}^4 \\ \text{C}^2\text{H}^3\Theta \end{matrix} \right\} \Theta^2$, and chlorbutyrine, $\left. \begin{matrix} \text{C}^2\text{H}^4 \\ \text{C}^4\text{H}^7\Theta \end{matrix} \right\} \Theta$,
ClCl

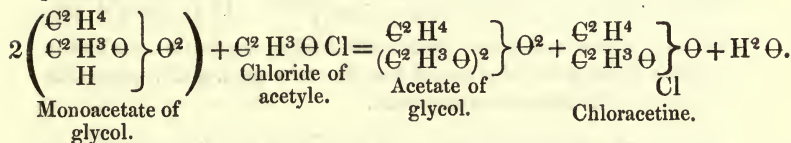
are obtained. The two phases of the action may be thus expressed:—



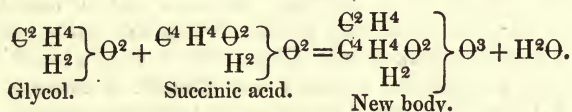
The reaction of a chloride of an acid radical on a glycol with one acid radical gives rise to the formation of an ether with two radicals and a chlorhydrine and water. By treating monoacetate

* Phil. Mag. vol. xix. p. 70.

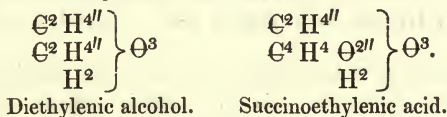
of glycol with chloride of acetyl, Lourenço has thus obtained chloracetine and acetate of glycol; and by treating monoacetate of glycol with chloride of butyryle, he has obtained acetobutyrate of glycol and chloracetine. The former reaction is thus expressed:—



The same chemist has investigated* the action of bibasic acids on glycol. He heated together succinic acid and glycol in a sealed tube to 90° for some hours. An action resulted which, as Lourenço anticipated, was in accordance with the equation



The contents of the tube, after the operation, consisted of a colourless, oily, homogeneous liquid. It was dissolved in alcohol, and a large quantity of ether added, by which some succinic acid was precipitated. After removing this, the alcohol and ether were expelled at the temperature of the water-bath, and the residue then maintained for some time at 200° in order to get rid of any excess of glycol. The analysis of the residual liquid gave for it the formula $\text{C}^6 \text{H}^{10} \Theta^5$. This body has a composition analogous to that of diethylenic alcohol†. It may be viewed as this compound in which an atom of the diatomic radical succinyle replaces an atom of the diatomic ethylene, and it may hence be called *succinoethylenic acid*. Thus



This acid is accordingly bibasic. The silver salt, $\begin{array}{c} \text{C}^2 \text{H}^4 \\ \text{C}^4 \text{H}^4 \Theta^2 \\ \text{Ag}^2 \end{array} \left\} \Theta^3$, is obtained by the double decomposition of succinoethylenate of ammonia and nitrate of silver as a gelatinous curdy precipitate. When succinoethylenic acid is heated to 300° it loses water, and leaves, on cooling, a crystalline mass which melts at about 90° C.

* *Bulletin de la Société Chimique*, p. 123.

† *Phil. Mag.* vol. xix. p. 124.

It melts in boiling water to a thick oil, which is insoluble in water, and may thus be freed from succinic acid. It is dissolved by boiling alcohol, from which it separates, on cooling, in small crystals. It decomposes by distillation. Analysis gave for it

the composition $\text{C}^6 \text{H}^8 \text{O}^4 = \left. \begin{matrix} \text{C}^2 \text{H}^4'' \\ \text{C}^4 \text{H}^4 \text{O}^2'' \end{matrix} \right\} \text{O}^2$. It is neutral *succinate of glycol*, and is the first well-defined example in which a diatomic radical is substituted for the two atoms of replaceable hydrogen of a diatomic compound.

Bauer has obtained * oxide of amylene, the compound corresponding to Wurtz's oxide of ethylene †. The preparation of this body is very troublesome, owing to the difficulty of preparing bromide of amylene, and, therefrom, amylglycol. To prepare bromide of amylene, amylene is placed in a long-necked globe, surrounded by a freezing mixture, and a calculated quantity of bromine gradually added. When the action is complete, the liquid is shaken with dilute potash, then washed with water, dried, and allowed to stand over chloride of calcium. It boils at 170° to 175° , but decomposes at that temperature. For the preparation of amylglycol, it is sufficient to keep it at 160° for some time. Bauer observed that this liquid contained some hydride of amyle, $\text{C}^5 \text{H}^{12}$, and some brominated amylene, $\text{C}^5 \text{H}^9 \text{Br}$, but both these pass off almost entirely below 100° .

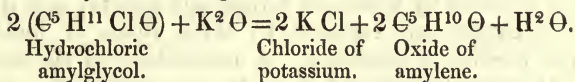
Bauer found, as has been observed by the writer of these notices, that acetate of potass cannot be used advantageously in the preparation of amylglycol. The action of bromide of amylene on acetate of silver is slow, but complete. It is effected by making acetate of silver into a paste with glacial acetic acid, gradually adding the bromide of amylene, and after the first action is over, heating the whole to 100° for a few days. It is then distilled, the portion above 140° being collected separately. When this is treated with potash and distilled, amylglycol is obtained.

When hydrochloric acid gas is passed into amylglycol, the liquid becomes coloured, and the further action of the gas converts it into a black viscous mass, which decomposes on distillation. Aqueous hydrochloric acid acts with great energy on amylglycol at ordinary temperatures, forming several chlorinated compounds, among which is hydrochloric amylglycol, $\left. \begin{matrix} \text{C}^5 \text{H}^{10} \\ \text{H} \end{matrix} \right\} \text{O}$. Efforts made to separate this body in the pure state were fruitless.

* *Bulletin de la Société Chimique*, p. 148.

† *Phil. Mag.* vol. xvii. p. 427 ; vol. xix. p. 122.

Oxide of ethylene was prepared by Wurtz by the action of potash on hydrochloric glycol; for the preparation of oxide of amylene, Bauer treated amylglycol with diluted hydrochloric acid, and then mixed the crude product thus obtained, which consisted mainly of hydrochloric amylglycol, with potash, and distilled the mixture. The distillate consisted of water with a layer of an oily liquid. This was removed, dried, and repeatedly rectified. It boiled at 95° , but could not be freed from traces of a chlorinated compound. The analyses and determination of the vapour density left no doubt that it was oxide of amylene, $\text{C}^5\text{H}^{10}\text{O}$, corresponding to oxide of ethylene, $\text{C}^2\text{H}^4\text{O}$. Its formation may be thus expressed:—



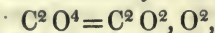
Oxide of amylene boils at about 95° , and has the spec. grav. 0.8244; it burns steadily with a yellow flame. It has an agreeable ethereal odour, and a bitter taste. It is insoluble in water, but dissolves in alcohol, in ether, in amylglycol, and in acids. Heated with acetic acid, it is converted into acetate of amylglycol.

In a series of recent communications, Kolbe has developed certain views of the constitution of several of the principal series of organic compounds. These views are supported by a wide variety of illustrations from organic chemistry; and several interesting investigations have been undertaken by Kolbe and his pupils in order to test their validity. Without going too much into detail it is impossible to give the grounds on which these views are based; but the investigations to which they have given rise will probably be intelligible from the following statement.

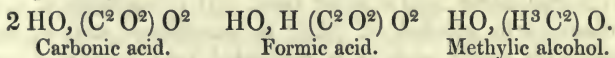
Organic compounds he considers to be derived from inorganic compounds, and usually resulting from them by simple processes of substitution. Carbonic acid plays the most important part among those inorganic compounds which serve as starting points for organic bodies. Liebig had already pointed out (in 1847) that many important compounds, alcohol, sugar, formic acid, stood in close relation to carbonic acid, and were derivable from it by the substitution of hydrogen for oxygen. It must be considered certain that starch, sugar, gum, the vegetable acids, &c., which are found in plants where the decomposition of carbonic acid takes place, are formed, at any rate indirectly, from this carbonic acid. This idea of the connexion between these bodies and carbonic acid is rendered antecedently probable by the relations of these formulæ, and by their mode of occurrence; and it has been made more probable since it has been found

possible to replace directly the oxygen in carbonic acid by hydrogen and bodies allied to it, and so give rise to the formation, not indeed of sugar, but of bodies immediately derived from it. In Wanklyn's discovery of the transformation of carbonic acid into propionic acid and into acetic acid, the view that the fatty acids, the alcohols, acetones, aldehydes, &c., are simple derivatives of carbonic acid, finds its chief support.

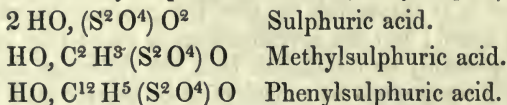
Kolbe writes the formula of anhydrous carbonic acid



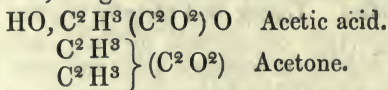
in which two atoms of oxygen are supposed to exist outside the radical carbonyle. If in carbonic acid one of the four oxygen atoms is replaced by hydrogen, formic acid results; and if three atoms of oxygen are replaced by three atoms of hydrogen, methylic alcohol is produced. A comparison of the formulæ shows these relations; and it is to be remarked that when a positive element replaces a negative, Kolbe writes it on the left of the primary radical.



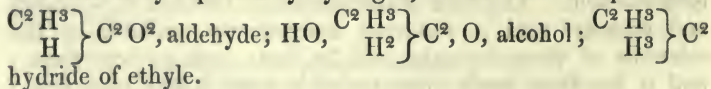
By the replacement of one of its atoms of oxygen, the bibasic carbonic acid is changed into the monobasic formic acid. Similarly, by the replacement of an atom of oxygen by methyle, ethyle, butyle, &c., we obtain the corresponding acids $\text{HO}, \text{C}^2 \text{H}^3 (\text{C}^2 \text{O}^2) \text{O}$, acetic acid; $\text{HO}, (\text{C}^4 \text{H}^5) (\text{C}^2 \text{O}^2) \text{O}$, propionic acid; $\text{HO}, \text{C}^8 \text{H}^9 (\text{C}^2 \text{O}^2) \text{O}$, valerianic acid. In an analogous manner, Kolbe derives from bibasic sulphuric acid, the monobasic methylsulphuric acid and phenylsulphuric acid.



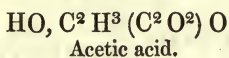
If in carbonic acid an atom of oxygen is replaced by an atom of hydrogen, acetic acid results; if a second atom of oxygen is replaced by a radical, we get the formula of an acetone.



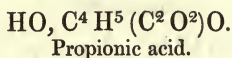
If, in carbonic acid after an atom of oxygen has been replaced by methyle, the second be replaced by chlorine, chloride of acetylene results; and if the second, third, and fourth atoms of oxygen be successively replaced by hydrogen, we have the compounds



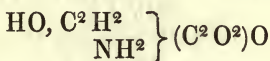
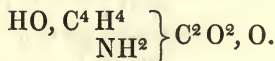
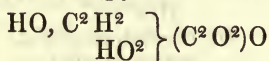
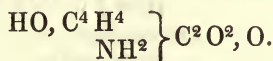
Glycocol, and its homologues alanine, leucine, &c., are derived from the corresponding acids, acetic, propionic, and valerianic, when an atom of amidogen replaces an atom of hydrogen. They hence receive the name amidoacetic, amidopropionic, amido-valerianic acids. In like manner Kolbe considers lactic acid to be derived from carbonic acid by the replacement of an atom of oxygen by an atom of peroxide of ethyle, or, as he calls it, oxyethyle; or it may also be regarded as propionic acid, in which an atom of hydrogen in the replacing radical is replaced by peroxide of hydrogen. Kolbe gives to it the name *oxypropionic acid*. The homologues glycolic and leucic acids have, of course, a similar constitution. Their relations will be best apparent from a comparison of their formulæ.



Acetic acid.

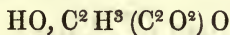
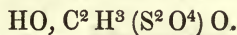


Propionic acid.

Amidoacetic acid
(glycocol).Amidopropionic acid
(alanine).Oxyacetic acid
(glycolic acid).Oxypropionic acid
(lactic acid).

The recent experiments on the conversion of lactic acid into propionic acid* and into alanine, support these relations.

In a similar manner Kolbe derives a series of bodies from anhydrous sulphuric acid, the formula of which he writes (S^2O^4), O^2 , that of the hydrated acid being $2\text{HO, (S}^2\text{O}^4)\text{O}^2$. Just as acetic acid is methylcarbonic acid, so sulphomethylic acid is methylsulphuric acid.

Methylcarbonic acid
(acetic acid).

Methylsulphuric acid.

Isethionic acid stands in the same relation to sulphovinic acid as lactic acid to carbovinic acid. And as lactic acid is carbonic acid in which an atom of peroxide of ethyle replaces an atom of extra-radical oxygen, so isethionic acid is derived from sulphuric acid by the substitution of an atom of oxygen for an atom of extra-radical oxygen. Kolbe adduces, in support of these views, some experiments which he has made with isethionic acid. When isethionate of potash is distilled with pentachloride of phosphorus, a compound, $\text{C}^4\text{H}^4 \left. \begin{array}{c} \text{Cl} \end{array} \right\} (\text{S}^2\text{O}^4)\text{Cl}$, corresponding to chloride of lac-

* Phil. Mag. vol. xix. p. 384.

tyle is formed*, from which he has further obtained chloroethylsulphuric and amidoethylsulphuric acids, and sulphovinic acid. Amidoethylsulphuric acid is identical with *taurine*. The relations of these bodies to each other, and to the corresponding terms derived from carbonic acid, are thus seen:—

$(C^2 O^2) O^2$ Carbonic acid.	$S^2 O^4, O^2$. Sulphuric acid.
$HO, C^4 H^5 (C^2 O^2) O$ Propionic acid.	$HO, C^4 H^5 (S^2 O^4) O$. Ethylsulphuric acid.
$\left. \begin{matrix} C^4 H^4 \\ Cl \end{matrix} \right\} (C^2 O^2) Cl$ Chloride of chloropropionyle (chlorolactyle).	$\left. \begin{matrix} C^4 H^4 \\ Cl \end{matrix} \right\} (S^2 O^4) Cl$. New chloride.
$HO, C^4 H^4 \left. \begin{matrix} \\ NH^2 \end{matrix} \right\} (C^2 O^2), O$ Amidopropionic acid (alanine).	$HO, C^4 H^4 \left. \begin{matrix} \\ NH^2 \end{matrix} \right\} (S^2 O^4) O$. Amidoethylsulphuric acid (taurine).
$HO, C^4 H^4 \left. \begin{matrix} \\ HO^2 \end{matrix} \right\} (C^2 O^2) O$ Oxypropionic acid (lactic acid).	$HO, C^4 H^4 \left. \begin{matrix} \\ HO^2 \end{matrix} \right\} (S^2 O^4) O$. Oxyethylsulphuric acid (isethionic acid.)

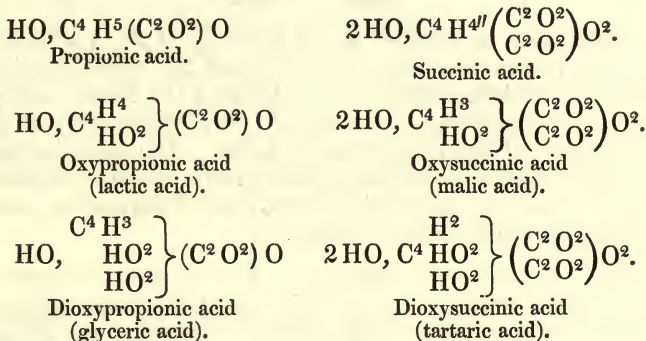
Kolbe extends these views to explain the constitution of some bibasic acids which stand in close relation to those of the acetic acid series. He derives these acids from the compound group, a double atom of carbonic acid, $\left. \begin{matrix} C^2 O^2 \\ C^2 O^2 \end{matrix} \right\} O^4$. When two of the extra-radical atoms of oxygen are replaced by a diatomic radical (for example, ethylene and homologous or corresponding carbohydrates), we obtain acids of the succinic series, and the analogous acids, phthalic acid, &c. Thus:

Succinic acid . .	$2 HO (C^4 H^4)'' \left(\begin{matrix} C^2 O^2 \\ C^2 O^2 \end{matrix} \right) O^2$.
Suberic acid . .	$2 HO (C^{12} H^{12})'' \left(\begin{matrix} C^2 O^2 \\ C^2 O^2 \end{matrix} \right) O^2$.
Sebacic acid . .	$2 HO (C^{16} H^{16})'' \left(\begin{matrix} C^2 O^2 \\ C^2 O^2 \end{matrix} \right) O^2$.
Pthalic acid . .	$2 HO (C^{12} H^4)'' \left(\begin{matrix} C^2 O^2 \\ C^2 O^2 \end{matrix} \right) O^2$.
Insolinic acid . .	$2 HO (C^{14} H^6)'' \left(\begin{matrix} C^2 O^2 \\ C^2 O^2 \end{matrix} \right) O^2$.

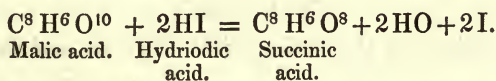
Now, malic and tartaric acids differ from succinic acid by containing respectively two and four atoms of oxygen more than

* Phil. Mag. vol. xviii. p. 285.

that acid. Kolbe considers that they bear to succinic acid a relation analogous to that which he considers lactic and glyceric acids to bear to propionic acid. That is, malic acid is *oxysuccinic acid*, and tartaric acid is *dioxysuccinic acid*, as is seen from the formulæ—



Schmidt has, from this point of view, made a series of experiments* on the action of reducing agents on malic and tartaric acids; and he has found that they may be converted into succinic acid by the use of the reducing agent, hydriodic acid, by which Lautemann† effected the transformation of lactic acid into propionic acid. The change was found especially easy with malic acid. When a concentrated aqueous solution of hydriodic acid was saturated with malic acid, and the mixture heated in a sealed tube for some hours to a temperature of 130°, a large quantity of iodine along with some brown crystals separated. The latter were succinic acid coloured with iodine. They were readily purified, and gave correct numbers on analysis. The change is thus expressed:—

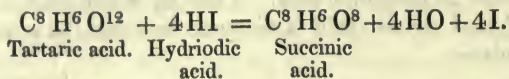


The transformation of tartaric into succinic acid took place with greater difficulty. It also was effected by saturating a concentrated aqueous solution of tartaric acid with hydriodic acid gas, and heating the mixture in a sealed tube to a temperature of 120°. The contents of the tube were then boiled in a retort, with frequent additions of water, until the iodine and hydriodic acid were removed, then evaporated to dryness, and treated with ether to remove the last traces of iodine, and finally, the succinic acid repeatedly crystallized from ether to free it from an admix-

* Liebig's *Annalen*, April 1860.

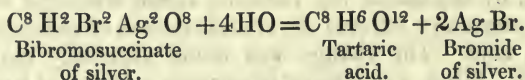
† Phil. Mag. vol. xix. p. 384.

ture of undecomposed tartaric acid. The change is thus expressed :—



Dessaigues, in a letter* to the Secretary of the Académie des Sciences, announces that he has effected the transformation of tartaric acid into succinic acid by treating the former acid with iodide of phosphorus. Dessaigues' view of the relations of these acids does not differ from those of Kolbe. The paper in which† the latter chemist developed his views, and in which he announced that Schmidt was occupied with the above experiments, appears to have been unknown to Dessaigues.

These experiments of Schmidt and of Dessaigues acquire an additional interest at the present time, when Perkins and Duppa‡, by an experiment the reverse of that of Schmidt, have converted succinic acid into tartaric acid. This has been done by means of bibromosuccinic acid. When the silver salt of this acid is boiled with water, it decomposes, forming bromide of silver and tartaric acid. Thus :



The reaction is thus exactly analogous to that by which chloroacetic acid is converted into glycolic acid.

V. On several Forms of Actinometer. By C. J. BURNETT, Esq. §

[With a Plate.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

21 Ainslie Place, Edinburgh,
April 13, 1860.

A PROPOS of the description of a "new actinometer" by Dr. Woods, which, as I find in its republication in the *Photographic Journal* of January 16, appeared *first* in your Magazine, I would call your attention to an *old* actinometer of mine, a rough sketch of which appeared in the *Liverpool Photographic Journal* of December 15, 1858. Without any insinuation

* *Comptes Rendus*, April 1860.

† *Liebig's Annalen*, March 1860.

‡ *Chemical Society*, April 19.

§ Communicated by Sir D. Brewster, K.H., F.R.S. &c.

of plagiarism against Dr. Woods, it appears right that you and your readers should know that the measurement of actinism by the gas evolved is not altogether so novel an idea as Dr. Woods imagines. By reference to my old sketch alluded to, you will find that not only the principle of measurement by gas-evolution, but more also of what Dr. Woods brings before the public as novel, has been anticipated; and had my sketch not been a very hurried one, abridged hurriedly from my old notes of 1855, it would have contained various other particulars which are, I believe, even yet new to the public,—and among others the following modifications, &c., which may perhaps be of interest.

I remain, Gentlemen,

Your obedient Servant,

C. J. BURNETT.

A column of oil, spirit, or other lighter liquid may be made (*entirely* or partially) to replace the mercury, the employment of this *lighter* liquid enabling us to use an erect register-tube without having the result so seriously interfered with by the pressure in it, as is the case where such a very heavy register-fluid as mercury is employed. The interference of the pressure of a column, and especially one varying in weight according to its varying height, gives rise, be it noted, to two inconveniences: first, the rendering of equal distances on the register-scale not true measures of the quantity of gas evolved, and of its pressure; and secondly, to a smaller and constantly decreasing sensitiveness of the instrument, owing to the smaller distance which the column, of the heavier liquids especially, is pushed forward as its height and pressure increases. There is an objection to water as a registry-liquid, that it absorbs and transmits a large portion of the carbonic acid, which neither oil, mercury, nor even, I think, spirit does. The employment of the sensitive fluid itself for an index, as suggested and employed by Dr. Woods, is manifestly still more objectionable, from the chemical action in this case going on in the register-tube as well as in the reservoir.

The oil or other liquid should be coloured, say dark red, to make the results more legible, or to facilitate the photographic registry of them, where that is wished to be carried on.

Using either the heavy or the lighter liquids, the apparatus, as figured formerly in one of its most simple states, may be with advantage modified in very many ways: *e. g.* Plate I. fig. 1 represents an apparatus in which the registry-liquid is contained in a second bottle or reservoir, A being the reservoir* containing the

* No attempt at correct proportion is made in these rough figures, which are only intended to show the principle of action. Thin glass, to intercept little light, would be advisable. N.B. The sensitiveness of the

gas-evolving or sensitive liquid, with, at the top, two openings, one for a cork or stopper, and the other for the tube C which conveys the gas to the second reservoir B, but which does not descend beyond the top of either reservoir. B is, as we have already stated, the second reservoir; and from the bottom of it starts the register-tube D, which may either be continued directly upwards, or slantingly, to diminish so far the pressure of the liquid, or may be carried along horizontally, so as to diminish it still further or eliminate it entirely, as in fig. 2,—the tube with the scale of degrees thus resembling so far the common register-thermometer (and if it should be necessary to prevent the liquids flowing onwards, though I hardly think it will when the tube is fine enough, we may have a short plug or cylinder close to the surface of the liquid, just as in the thermometer alluded to).

Fig. 3 represents a plan (which will be found to answer well enough without resorting to an additional reservoir and its accompaniment, just described) to prevent the mixture of the register-liquid with the sensitive liquid; and which is quite necessary where oil is the fluid used for the former, as otherwise it would come to the top of the sensitive liquid, instead of lying at the bottom in contact with, and ready to be forced up, the register-tube, as a heavier fluid might do. In fig. 3 the scale is most conveniently attached to, and the registry made by, the side of the tube next the reservoir; and the superfluous register-liquid, as it is driven onwards, may then be allowed to flow out at the end of the tube, which may be turned down so as to prevent its accumulating in a column above, and acting by its pressure; to get rid of which source of error and of diminished sensitiveness, we may mention (adhering still to the same general system of registry by production of gas) two other plans. They are, first, the substitution (in an upright or other tube) of a solid, closely-fitting cylindrical piston* for the varying column of the register-liquid (the piston having, it may be, a little oil or other liquid above it to show that there is no leakage of gas). Such a piston (or we may call it a cylindrical plug, as it does not necessarily have any rod attached to it), being *constant* and uniform in its height and resistance, will, if well fitted and running smoothly, give a uniform resistance, and consequently be an accurate index of the amount of gas liberated. If in a glass tube, it will (even with

apparatus is in proportion to the size of the sensitive reservoir and the bulk of its liquid exposed to sunshine. A good plan of facilitating the regular rising of the liberated carbonic acid, is to put a little sand or broken glass (of a given weight) into the sensitive liquid before solarization.

* Say of some very light material, as pith, cork, gutta-percha, india-rubber, to diminish resistance to the pressure of gas, especially when the tube is upright.

the film of oil above it) be as convenient for *photographic* registry as the column of liquid was; and if in a metal one, photographic (or other) registry might be carried out by the intervention of a piston-rod and other easily contrived apparatus attached, or by attaching an upright glass tube with a column of liquid, to the top of the metal one; and such a column of liquid, being not fluctuating in height like those in our earlier-described forms, but of one constant height, will give a correct register, as far as absence of unequal pressure goes. (This suggestion of a metal tube with a glass one over it, is made in case it should be found difficult to fit the glass tube sufficiently well to the piston.)

The second plan is the making our register (photographic, or simply visible) dependent on the loss of weight experienced by the gas-evolving liquid, the vessel containing the sensitive liquid being either balanced by a delicate spring, or by a system of leverage, on some plan allied to the steel-yard principle; or else, and probably better, the vessel containing the sensitive liquid is made to float at a fixed height (when charged) in or above the surface of water or other liquid in a suitably-shaped vessel, and the loss of weight, and consequently the amount of actinic influence exerted, is measured by the height to which, or rapidity with which, it rises after exposure or during exposure to light. This form of apparatus has a considerable resemblance to some of the hydrometers. The apparatus may of course be arranged either so as to leave the sensitive-liquid-containing vessel below, or so as to keep it above the liquid. Above should have the advantage in the sensitiveness of the apparatus, by not losing so much light by absorption; while under has the advantage of enabling us to have our thermometer in the water close beside it, and so either to register the temperature, if we wish, on the same sheet of paper on which the actinism is registered, or else to maintain the temperature of the sensitive liquid uniform* by letting a constant flow of cold water pass through the water vessel; or rather, to prevent shakings and disturbance of the registry, we may have the sensitive(-liquid) vessel, with its connexions, protected from disturbance by the motion of the current of cold water by an immediately surrounding glass vessel or thin tube. The immersion of the sensitive vessel would also be convenient, as enabling us to make many interesting observations connected with resistance offered to actinic rays, or modifying effects produced on them, either by variously coloured, or by colourless, liquids of various compositions, as solutions of

* A set of observations with the sensitive vessel at different temperatures would be in many ways likely to lead to interesting results, and could probably be easily managed by regulating the heat of the current passed round the sensitive vessel.

vegetable and mineral salts, &c., fluorescent and non-fluorescent, variously polarizing and non-polarizing, &c. Such observations might also probably give interesting results as connected with substances of one composition, but differing allotropically. Using various liquids in this way, we should, however, of course require to make allowance for the effects of differing specific gravities, either in reading or in translating the observations, or otherwise*.

As to the precise form of this apparatus and mode of registering, whether photographic or other, the principles once explained, it would be occupying space to perhaps no great purpose to enter into pretty obvious details and variations. If the sensitive-liquid reservoir, if below the water (or the float attached to it, if above), has attached to it a *descending* rod which fits into, and is held *perpendicular* by, a ring or rings, and if another rod attached to the float or sensitive-liquid vessel *above* the water is *similarly* kept in its *upright* position by similar rings, it is obvious that the upper rod affords very convenient means of registry, either by having the scale on itself, with a marker pointing to it attached to the side of the outer vessel or other fixed object, or by making it carry the marker (in this case a shifting one) and making the scale attached to the outer vessel (or otherwise) fixed and steady. As to the adaptation of photographic self-registry on sheets or slips of sensitive paper, I need hardly say that the upper rod is capable of performing here exactly the same part which the column of mercury, oil, or other register-liquid performs in the earlier-described apparatus.

With regard to all the apparatus described, it would be in most cases, especially where used for protracted observation, advisable to have them kept in one angle, as regards the sun, by some sort of heliostat. It might, however, be found practicable in consecutive, independent, and even perhaps in continuous observations, to avoid the necessity of this by having the sensitive vessel, or that part of it which is exposed to the rays, of a globular form, so as to make their incidence similar at all hours. In this case, however, it must be observed that *the part* exposed to the rays must be filled with liquid; otherwise, however similarly they might fall on the surface of the *glass*, we should still have them impinging at varying angles on the plane upper surface of the contained sensitive liquid. Where the weight and floatation-system is employed, this plan is not quite so conve-

* We might avoid this, or the necessity for other scales, simply by adding to, or deducting from, the weight of the sensitive fluid and its reservoir till it floats or sinks to the same depth in the liquid to be experimented on as it does in water. This will be conveniently managed by having a little cup attachable to some part of the floating apparatus, into which we may put more or less sand, removing or adding it till the apparatus floats at the proper line marked on or by it.

nient; but if we estimate the weight in any other way, the difficulty disappears.

With regard to all these modes of registry by gas-evolution now described, there are two material deficiencies, at all events as applied to *continuous* self-registry. The first is the constant diminution of sensitiveness of the apparatus, consequent on the diminishing quantity of the decomposable salt or salts left in the liquid; and this (though the inconvenience might possibly be to some extent diminished by using a very large quantity of solution with only a small portion of it exposed to the light, and arranged so as to secure a circulation of liquid, or by having a supply of a concentrated solution of the salt or salts gradually admitted, and there are also other possible contrivances), together with the uncertainties about the influence of absorbed light at the beginning of, and after withdrawal from, solarization, about absorbed gas at the beginning of solarization, and about so-called catalytic influence, or the impulse which changing particles give to others, renders it necessary to speak cautiously about the value of at least continuous observations with one and the same portion of liquid, till careful experiment has shown what sort of results are attainable.

As to repeated observations with successive and fresh portions of liquid, we can of course feel *much more* confident of valuable results,—photographic or other self-registry, if desired here, however, necessitating the recourse to rather more complex apparatus.

In my old paper, the salts recommended for forming the sensitive solution were those of ferric and uranic oxide,—the binoxalate, ammonio-oxalate, and soda-oxalate of ferric oxide, and the binoxalate of uranic oxide being especially suitable. For convenience, I tried also the mixture of some of the more generally accessible salts, as uranic nitrate, or *iron-alum*, with oxalic acid or binoxalates*. Some of my other experiments were with solutions of chromic acid, or bichromate of ammonia, soda, or potash, along with oxalic acid, or one of its acid or neutral salts. Citric, tartaric, and other vegetable acids were tried instead of oxalic, but they seemed in many cases to be much acted on even in the dark. Solutions of brown oxide of manganese in tartaric, citric, and, still far more, in oxalic salts were liable to the same objection, as well as to that of uncertainty as to strength. A solution of permanganate of potash along with one of these vegetable acids or of its salts (or ammoniacal salts†?), however, seems not altogether so unpromising‡.

* The presence of an excess of oxalic or other acid is perhaps useful.

† Nitrogen being liberated.

‡ Solutions of ferric salts containing citric, tartaric, or other vegetable acids, rendered alkaline by ammonia, and with the iron held in solution,

We now come to the *paper actinometer*, which offers the very great advantage that the sensitive substance and the registering material are one and the same. Nothing, in fact, can well be simpler than the apparatus. The sensitive paper, ruled with lines to correspond with the seconds and minutes and hours, days, weeks, and months, or whatever other intervals are to be observed, is made by clockwork to pass (either firmly fixed and revolving on one roller of large diameter, or otherwise) behind a hole or slit which admits the light, the intensity of which will be of course registered by the differing degree to which the different parts of the paper are affected by it*.

The main difficulties attending this system of actinometry are, 1st, the difficulty of securing uniform sensitiveness in the same, and still more in different samples of paper, even when similarly sensitized; 2nd, corresponding uncertainties in development, where development-processes are used; 3rd, the difficulty of keeping paper unaltered in its sensitive condition, and this both *before* and after the solarization (as we may wish to develop several sheets at once, to be sure that the developer acts alike on them); and 4th, the difficulty of different papers being differently acted on by the fixing solutions. Still all these difficulties may to a great extent be got over by care and proper contrivances in the selecting, sensitizing, drying, and development of papers, cutting off parts near the edge, and using for each observation, not one single slip, but several, say two or three, cut either from different sheets or different parts of the same sheet, or, if from originally contiguous parts, with the sides (*not* the *surfaces* of the paper) of one of them reversed, so that the original right-hand of the one slip, or its top part, corresponds to, and is, *when in the actinometer*, in contact with, or juxtaposition to, what was *originally* the left-hand side of the other slip, or the lower part of it, if they were cut from top to bottom of the sensitive sheet.

As to the third source of difficulty attending this variety of actinometer (for in the others, where the paper is *only* the registering machine, the exact depth of tint produced is of no moment, the direction or curve of its line being all that is required), there would appear to be modes of sensitizing the silver papers which give a much better paper as regards keeping than others. It

showed little action. The addition of a neutral chromate, however, made them much more sensitive; and *after* this addition they were found capable of yielding very sensitive photographic papers, giving good impressions, capable also of further development by silver-salts, metal-cyanogen salts, &c.

* To eliminate possible action of actinism held in solution by the air or other actions of the air, it might be well to compare the record of the instrument when the paper in the solarization-slit is open to the air, with the action when it is protected by a *thin* scale of glass. This comparison might possibly lead to very interesting results.

may be generally observed that, using the ordinary style of papers, the best plan, where the paper is intended for a long observation, is not to use development, and not to make it too highly sensitive, also to sensitize it as if it were a very weak paper for the ordinary positive printing. Where, however, it is intended for registry of seconds or minutes, we must use a more highly sensitive printing-paper, or have recourse to an after-development of it; and it is here also, when we wish to have sets of *successive* observations made from slips cut from *one* sensitive sheet (to ensure uniform value of colour), that our difficulty is felt.

Suppose we have two, three, or four slips, each one representing, say, a different hour with the variations during it*. Here there may be no very great difficulty in one way, as supposing the hours successive, there is hardly time for the paper to go far wrong; but suppose they are successive *days* instead, we see the relative value of the periods on each day, but are at a loss to estimate their value compared with another day. The simplest plan which suggests itself is to check the value of the hour observations by day observations, of the day observations by week observations, and so on,—the observations of the longer periods being made, as they conveniently can be, on much weaker and consequently much better-keeping paper; and these different sets of observations will enable us to eliminate not only errors produced by the sensitive paper going wrong by keeping, but original differences in the quality of the papers before sensitizing, as well as in the chemicals, and, what with all care is most difficult to avoid, in the manipulation and in the weather.

As to the fourth difficulty (that connected with the inequality in fixing), with all care and study of time, and keeping the liquid in motion, it is a serious one; for, though we may secure pretty uniform action by these precautions and by study of strength and temperature in the case of one piece of paper, yet in the case of comparisons between different papers we must trust a good deal to the system of mutual checks (as of day papers or hour papers, &c.), as is to be explained.

Another plan, however, is, to be satisfied with a less perfect fixing, which will do well enough to engrave from, or copy in China ink, or to print from by a negative process, or to make a negative from in the camera.

For some of our *registers* by the Papyro-actinometer, the gradually and *uniformly* progressive motion behind the solarization-slit may be the best; but for others a motion by jerks,

* Or, as formerly described, we may and should have two or more slips (say reversed, as described, for each hour); but we have left this out for simplicity in the explanation.

with still intervals between, might be preferable ; and this latter plan would give us, in *different* registers, the average of each second in the minute, each minute in the hour, hour in the day, day in the week or month, and so on,—the average or mean, as compared with other days on the same week's or month's registry-slip, giving us at once the true value of the depth of tint in the whole and *every part* of that day's *own separate* registry, and so giving it a real utility which, taken *by itself*, and without being thus brought to the test, it could never have had ; and (on the same principle) so on with week-, month-, and, it may be, year-register papers. It must be allowed, with all their recommendations, that it would be desirable to get papers which would keep better than the ordinary silver ones, and which would suffer less in fixing.

As long ago as 1857* I called attention to the probable value, both for actinometry and for all other self-acting scientific registry by time, of papers prepared with the salts of ferric and uranic oxides. I had then, as I remarked, found them to retain their properties unimpaired for between one and two years, and I have since found them good when four years old. This ought to save a good deal of labour, by enabling us to prepare at once and keep ready a large stock of them. I am not, however, prepared to state what their relative advantages as compared with silver papers are, as regards keeping *after solarization* and before development. The development is, I think, more uniform in its results than that of the silver papers, being performed with nitrate of silver alone. The time of keeping without a material change after solarization (or the amount of change) would require to be more minutely examined before I could speak very positively as to the advantage of their application to the papyro-actinometer ; but for the paper-registry of the gas-evolving actinometers, whether gauged by floating or by fluid columns, as well as for self-registry of thermometers, barometers, anemometers, &c., there can be, I think, little room to doubt that these papers will be found particularly convenient.

It is perhaps hardly necessary to remark that one clock, or other moving power, may of course be made to move all the different time-registries we have recommended, each one at its own proper rate.

A very convenient arrangement for a set of paper-slips would be to have one a continuously moving slip ; the second one moving by sudden jerks, taking place every minute or every five minutes, and so giving the average of, say each minute or *five minutes* ; the third a similarly jerked slip, giving the average or mean for each hour ; and the fourth (say a *week-* or month-

* Even in 1855 I alluded to the possibility of their availability.

long slip) giving the average or mean for each *day* of twelve or *twenty-four* hours*.

In conclusion, I would call attention to the great importance of carrying on a system of actinic registry coupled with simultaneous observations of other kinds, including ozone observations (though I believe there is much uncertainty as yet attached to the latter†), (1) in various latitudes, and with reference to the

* Divisions by tens instead of twelve, might in some respects be preferable, and sets of slips might conveniently, for several reasons, be all of one length; among other advantages of this uniformity, we could use the same papers for all. An interesting set of observations would be on the actinisms before and after sunset, and on the effects of moonlight (and, query, starlight?); but then we should require either to observe the effect of *longish periods*, or to employ very sensitive papers, or daguerreotype, or else to concentrate the rays on the solarization-slit by a powerful lens. As to the probable superiority of either mode of estimating actinism, *i. e.* by the amount of its action on sensitive paper, or by that on sensitive liquids (or, it may be, on gases, such as carbonic oxide, and chlorine, &c.), their relative advantages will probably be found to vary considerably according to varying circumstances. For a continuous observation there are certainly (unless we use more complicated apparatus) some apparent advantages attending the estimation by sensitive paper alone; and in many cases it might be found possible to preserve a tolerably accurate record of the relative depths of tint produced (without the trouble and risk attendant on keeping, hypo-fixing, and printing from the impressed papers), by providing ourselves with a scale of permanently tinted paper, ivory, or other material representing a great variety of depth of tint gradually increasing from one end of the scale to the other, and each depth of tint being marked with a number, 1, 2, 3, 4, &c. We should only (after some *slight* and *temporary* fixing process, sufficient to prevent rapid action of light during comparison) require to shift each part of the actinized paper along the scale till we came to a part corresponding to it in depth of tint, and then record the number (1, 2, 3, or others) which represents it. There is, however, a difficulty attending this plan—that the *colour* of the actinized paper may turn out to be different from that of the tints on our scale, so that it may not be found easy to say what is the depth of tint corresponding. This difficulty, however, might be met or obviated in various ways. 1. By having several similar scales of different colours, such colours being the same as those which our sensitive paper is found liable to assume. 2. By employing such photographic papers, &c. (developments if necessary), whether argentine, ferric, uranic, chromic, or others, as are least liable to vary in colour, either when used on different occasions or on one occasion in the one slip, according to strength of actinism. 3. By making even our *one* scale vary to a certain extent in the nature of the colour, as well as in depth of it, if the difficulty just alluded to in connexion with this should still be found troublesome. My impression, however, is that there are photographic papers which will be found to colour so uniformly as to render this variation on our scale unnecessary.

† How, for instance, can we distinguish between the effects of loosely absorbed actinism and ozone? and how are we sure that the actions of the tests themselves may not produce it, and more rapidly in certain states of the air than in others?

corresponding latitude in the two hemispheres ; (2) in countries in the same latitude but differing in climate, *e. g.* in America and in Europe, and generally on the opposite sides of continents, and on sea contrasted with land ; at sea in the trade-wind belt and in the variables, &c. ; in our own country, on the west coast as contrasted with the east ; on the sea coast, and inland ; amidst vegetable life, and away from it, and the latter especially as connected with the disease known as “ hay fever,” possibly produced by the abundance of ozone or some *imponderable*, or *wave-power*, disengaged by vegetation during what we may call the *phytolysis** of carbonic acid—the supposition of such injurious effect, on persons exceptionally constituted, of allotropic oxygen or some unrecognized wave-power or imponderable or other agent disengaged or produced by vegetation being no way inconsistent with the belief that this same liberation or production is, in general, importantly or essentially conducive to the purification and salubrity both of our own atmosphere and of that watery one in which organic impurities would have a still greater tendency to accumulate ; (3) at different elevations in the same latitude and country, either on mountains or, occasionally, in balloons.

Such observations would be likely† to help us to a somewhat less utterly hazy idea than what we have as to *what constitutes climate*, and what is the nature of its action on vegetable, and what on animal life. Why does a plant or animal thrive in one country and languish or die in another, where there are not sufficient differences of soil, moisture, or temperature to account for the difference between life and death, luxuriance or languishing ?

* Phytactinolysis would be unnecessarily clumsy, and actinolysis I would reserve for the numerous cases where actinism acts without the presence or aid of vegetable or other life.

† There are many other considerations, however (as to probable actions of soil, &c. on the air), to be taken into account. Is the very remarkable and well-ascertained difference in sanitary properties between the air of the Egyptian valley and that of the *immediately* adjoining portions of the desert in any degree owing to this, or to the effect of vegetation ? It would seem, also, in the absence of any other probable cause yet given, deserving of investigation whether the remarkable properties of the well-known desert *simoom* may not be owing to allotropic action, to wave-powers, or imponderables connected either with chemical actions, or with friction between the sand and the air at certain temperatures : and why may not other influential powers as well as heat, light, and electricity be disengaged by the friction of bodies ? and why may not the necessary accompaniments and circumstances be as peculiar as they often are with relative electricity ?

VI. *Notices respecting New Books.*

Arithmetic in Theory and Practice for advanced Pupils. Part the First. By J. BROOK SMITH, M.A. London: Macmillan and Co. 1860.

ALTHOUGH Mr. Smith's arithmetic can scarcely be said to meet any urgent demand, inasmuch as we already possess several excellent works of the same kind, it deserves to take its place amongst the small number of sound educational works. The first part, the only one yet published, contains a lucid and correct exposition of the principles of numeration, addition, subtraction, multiplication, and division, of the theories of the greatest common measure and least common multiple, and of some of the more elementary properties of numbers with respect to their divisibility.

In such a treatise we do not, of course, expect to find new matter; our demands are limited to a few necessary qualities, such as clearness, precision, conciseness, good arrangement of the several parts, and at least so much originality in the general treatment of the subject as is necessary to support the book's claim to a separate existence. With respect to the two first of these qualities, this treatise has far more than average merits; in the third quality it does not excel so much, for the numerous theorems scattered throughout the book might, we think, have been better "welded" together. In point of arrangement there is much merit and some originality, a greater space than usual having been devoted to the very useful and interesting subjects of the criteria of the divisibility of numbers and of their decomposition into prime factors.

With respect to general treatment, one of the author's chief objects has been to render his demonstrations purely arithmetical. To do so he has been induced to discard all symbols and to conduct his proofs by means of particular numbers, striving always to make the general law apparent in the particular example. In some cases, and for young pupils especially, this method has its advantages, but in other cases it is clumsy and open to suspicion. Mr. Smith appears to have mistaken a little the true character of an arithmetical, as distinguished from an algebraical proof; he must admit that symbols *may* be introduced without interfering in the least with the purity or the strictly arithmetical character of the reasoning. Thus used, symbols are merely representatives of the subjects (numbers) discussed; no material diminution in the length of the proof is produced by their introduction, but the latter gains in elegance, in generality, and sometimes even in simplicity. Some of the author's proofs might, we think, have been improved in this manner.

Mr. Smith professes to write for advanced pupils, but in all probability his work will find the greatest number of readers amongst junior masters, the advanced pupils of our schools being in general too much occupied in breaking new ground to be able to return and inquire so closely into the principles upon which the very rudiments of their science is based. To the junior master, however, this re-examination is absolutely indispensable: he has to launch the young intellect entrusted to his care into the wide ocean of mathematics,

where at the very outset there are numerous shoals and reefs to be carefully avoided, and the success of the voyage depends greatly upon the manner in which it is commenced. To junior masters and pupil-teachers, therefore, Mr. Smith's book will be both acceptable and valuable: alone it will by no means fit them for their duties, but as a guide in the preparation of their own lessons it may be of great utility.

Of Motion. An Elementary Treatise by JOHN ROBERT LUNN, M.A., Fellow and Lady Sadleir's Lecturer of St. John's College. Cambridge: Deighton, Bell and Co. 1859.

Mr. Lunn, in his Preface, says,—“My object in the following pages has been to put forth the principles of the science of motion in their true geometrical form, postponing the consideration of force (the properties of which are presumed to have been fully investigated in statics) until the reader may be able to separate in his mind the geometrical ideas from the mechanical. To the fact that these ideas are not kept separate at the outset, I apprehend that the want of clearness in the student's mind about the real investigation that does take place in any case may be attributed.”

We very much doubt whether the difficulties encountered by those who are commencing the study of dynamics are—to any great extent—assignable to the cause here alleged. Nor do we understand how “the properties of force” can be “presumed to have been *fully* investigated in *statics*,”—seeing that in “statics” only *some* of the properties of force, and those the least important, are investigated. Nevertheless we doubt not that Mr. Lunn's mode of treating the subject will be useful to *some* readers, whilst his numerous problems and their solutions will certainly be so to *all*.

A Treatise on the Calculus of Finite Differences. By GEORGE BOOLE, D.C.L., Professor of Mathematics in the Queen's University, Ireland. Cambridge: Macmillan and Co. 1860.

A Treatise on Attractions, Laplace's Functions, and the Figure of the Earth. By JOHN H. PRATT, M.A., Archdeacon of Calcutta. Cambridge: Macmillan and Co. 1860.

The name and character of Professor Boole are too well known to our mathematical readers to render it necessary for us to do more than announce this new work of his ‘On the Calculus of Finite Differences.’ As an original book by one of the first mathematicians of the age, it is out of all comparison with the mere second-hand compilations which have hitherto been alone accessible to the student. We heartily wish that our elementary books on mathematics were more frequently written by men of the rank of Prof. Boole, instead of being left to mere book-makers.

The work of Archdeacon Pratt “is in part a republication of those portions of” his “work on ‘Mechanical Philosophy’ which treat of Attractions, Laplace's Functions, and the Figure of the Earth;” but very greatly enlarged and improved. The reader will find new and interesting matter, both in the purely analytical portion and in the physical applications. “It has been my endeavour,” says the author, “to put the well-known difficulty in Laplace's analysis, arising from

the use of a discontinuous function, in the clearest light, that the student may understand both what it is and how it is overcome. . . . I have also introduced some propositions on the geodetic method of determining the Figure of the Earth, suggested by an acquaintance with the circumstances of the Great Trigonometrical Survey of India, and by the volume of the Ordnance Survey of Great Britain and Ireland recently published by Lieut.-Colonel James, R.E., Superintendent of the Ordnance Survey." The researches of Mr. Hopkins and Professors Hennessy and Haughton are also referred to. As a specimen of some of the calculations in this volume, interesting to the general reader, we may cite the following:—"The increase in height of the sea-level at Karachi above that at Cape Comorin, arising from the drawing of the waters northwards by the Himmalayas, and in consequence of the deficiency of attraction of the ocean=643 feet."

VII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xix. p. 467.]

December 22, 1859.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

THE following communications were read:—

"On the Electric Conducting Power of Alloys." By A. Matthiessen, Ph.D.

In this paper I have given the determinations of the electric conducting power of upwards of 200 alloys, and have found that the metals employed may be divided into two classes, viz.—

A. Those metals which, when alloyed with each other, conduct electricity in the ratio of their relative volumes.

B. Those metals which, when alloyed with one of class A, or with each other, *do not* conduct electricity in the ratio of their relative volumes, but *always less*.

The alloys may be divided into three groups; viz.—

1. Those made of the metals of class A with each other.
2. Those made of the metals of class A with those of class B.
3. Those made of the metals of class B with each other.

From the experiments described in the paper I have tried to deduce the nature of alloys, and have arrived at the following conclusions:—

A. That most alloys are only a solution of one metal in the other; for,—

1. On looking at the curves belonging to the different groups, we see that each group of alloys has a curve of a distinct and separate form. Thus for the first we have nearly straight lines; for the second, the conducting power decreases always rapidly on the side of the metal belonging to class B, and then turning, goes almost in a straight line to the metal belonging to class A; for the third group we find a rapid decrement on both sides of the curve, and the turning-points united by almost a straight line.

2. On examining that part of the curve where the rapid decrement

takes place, we find that with the lead and tin alloys it generally requires twice as many volumes of the former as of the latter to reduce a metal of class B to a certain conducting power.

3. That the turning-points of these curves are not chemical combinations we may assume from the fact that they only contain very small per-centages of the one metal.

4. That the alloys at the turning-points have their calculated specific gravities.

5. From the similarity of the curves of the conducting power of alloys, where we may assume we have only a solution of one metal in the other, we may always draw approximatively the curve of the alloys of any two metals if we know to which class they belong.

B. That some alloys are chemical combinations; for—

1. At the turning-points of the curve we generally find contraction or expansion.

2. We have no regular form of curve, so that we cannot *a priori* approximatively draw it.

3. At the turning-points of the curve, the alloy retains large per-centages of each metal.

4. The appearance (crystalline form, &c.) of the alloys at these points is different from each other.

C. That some alloys are only mechanical mixtures; for example, bismuth-zinc, lead-zinc, some silver-copper alloys, &c.

The question now arises, To what is the rapid decrement of the conducting power of the metals belonging to class B, when alloyed with other metals, due?

The only answer I can at present give to this question is, that most of their other physical properties are altered in a like manner; for where we find no marked change in most of the physical properties, as in group I, and in the second group on the one side of the curve, then we have nearly their calculated conducting powers.

In the Appendix I have given some determinations of the conducting power of pure gold, which I find has a higher value than that generally quoted.

In conclusion, I may take this opportunity of thanking Dr. M. Holzmann for the excellent manner in which he has carried out the greater part of the experiments.

“On an extended Form of the Index Symbol in the Calculus of Operations.” By William Spottiswoode, Esq., M.A., F.R.S.

“Problem on the Divisibility of Numbers.” By Francis Elefanti, Esq.

“On the Structure of the *Chorda Dorsalis* of the Plagiostomes and some other fishes, and on the relation of its proper Sheath to the development of the Vertebrae.” By Professor Albert Kölliker.

“Remarks on the late Storms of October 25–26 and November 1, 1859.” By Rear-Admiral FitzRoy, F.R.S.

As many of our Society must doubtless be interested in the nature and character of that storm in which the ‘Royal Charter’ went to

pieces on Anglesea Island, and as abundant information has been obtained from Lighthouses, Observatories, and numerous private observers, I would take this earliest opportunity of stating that the combined results of observations prove the storm of October 25th and 26th to have been a complete horizontal cyclone.

Travelling bodily northward, the area of its sweep being scarcely 300 miles in diameter, its influence affected only the breadth of our own Islands (exclusive of the west of Ireland) and the coast of France.

While the central portion was advancing northward, not uniformly but at an *average* rate of about twenty miles an hour, the actual velocity of the wind—circling (as against watch-hands) around a small central “lull”—was from forty to nearly eighty miles an hour.

At places north-westward of its centre, the wind appeared to “back” or “retrograde,” shifting from east through north-east, and north to north-west; while at places eastward of its central passage, the apparent change, or veering, was from east, through south-east, south, south-west, and west.

Our Channel squadron, not far from the Eddystone, experienced a rapid, indeed almost a sudden shift of the wind from south-east to north-west, being at the time in, or near, the central lull; while, so near as at Guernsey, the wind veered round by south, regularly, without any lull. This sudden shift off the Eddystone occurred at about three (or soon after), and at nearly half-past five it took place near Reigate; westward of which the central lull passed.

From this south-eastern part of England, the central portion of the storm moved northward and eastward. Places on the east and north coasts of Scotland had strong easterly or northerly gales a day nearly later than the middle of England. When the ‘Royal Charter’ was wrecked, Aberdeen and Banffshire were not disturbed by wind; but when it blew hardest, from east to north, on that exposed coast, the storm had abated or almost ceased in the Channel and on the south coast of Ireland.

Further details would be ill-timed now, but they will be given in a paper to the Royal Society, as soon as additional observations from the Continent, and from ships at sea, have been collected and duly combined with other records.

The storm of the 31st, and 1st of November, was similar in character; but its central part passed just to the west of Ireland’s south-west coast, and thence north-eastward.

Of both these gales the barometer and thermometer, besides other things, gave ample warning; and telegraphic notice might have been given in sufficient time from the southern ports to those of the eastern and northern coasts of our Islands.

As it is the north-west half of the cyclone (from north-east to south-west, *true*) which is influenced chiefly by the cold, dry, heavy, and positively electrified polar atmospheric current, and the south-west half that shows effects of equatorial streams of air—warm, moist, light, and negatively electrified;—places over which one part of a cyclone passes are affected differently from others which are traversed by another part of the very same meteor, or atmospheric

eddy, the eddy itself being caused by the meeting of very extensive bodies of air, moving in nearly, but not exactly opposite directions, one of which gradually overpowers, or combines with the other, after the rotation.

On the *polar* half of the cyclone, continually supplied from *that* side, the visible effect is a drying up and clearing of the air, with a rising barometer and falling thermometer; while on the equatorial side, overpowering quantities of warm moist air—rushing from comparatively inexhaustible tropical supplies—push towards the north-east as long as their impetus lasts (however originated), and are successively chilled, dried, and intermingled with the always resisting, though *at first* recoiling, polar current. After such struggles these two currents unite in a *varying* intermediate state and *direction*, one or other prevailing gradually.

Very plain and practical conclusions are deducible from these considerations:—

One, and the most important, is that in a gale which seems likely to be near the central part of a storm, that should be (of course) avoided by a ship which has sea room: a seaman, facing the wind, knows that the centre is on his *right* hand in the northern hemisphere, on his *left* in the southern; he therefore is informed *how to steer*.

Another valuable result is that telegraphic communication can give notice of a storm's approach, to places then some hundred miles distant, and *not otherwise forewarned*.

Jan. 12, 1860.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

The following communications were read:—

“Notes of Researches on the Polyammonias.”—No. VII. On the Diatomic Ammonias. By A. W. Hofmann, LL.D., F.R.S.

In continuing my inquiries into the nature of the organic bases, I was led in the commencement of the year 1858 to repeat some experiments on the action of dibromide of ethylene upon ammonia, which M. Cloëz* had published in 1853. The repetition of these experiments compelled me to contest not only the formulæ of M. Cloëz, but also the general interpretation which he had given to his results.

I have not hesitated to communicate my conclusions to the Royal Society†.

M. Cloëz‡ shortly afterwards discussed my observations, and pointed out the arguments which induced him to maintain his formulæ and his interpretations.

I have not replied to these remarks. M. Cloëz having stated in the same note that he was still engaged with his experiments and that his inquiry was nearly completed, I discontinued my experiments on the action of dibromide of ethylene upon ammonia, fully persuaded that the chemist, to whom we are indebted for the first observation of this reaction, in continuing his experiments would arrive at the same results which I had myself obtained.

In discontinuing the discussion with M. Cloëz, I was not freed from the obligation of proving the general thesis of my note, viz.

* L'Institut, 1853, p. 213.

† Phil. Mag. vol. xvi. p. 309.

‡ Comptes Rendus, xlii. p. 255.

the formation of diatomic bases by the action of diatomic bromides on ammonia. I have given the proof in several communications* addressed during the last two years to the Royal Society, and especially in a note† describing some new derivatives of phenylamine and ethylamine published during last summer. The formation of these bodies, their analysis and their transformations, have, I believe, settled the question at issue in a satisfactory manner.

These researches have been the subject of some remarks on the part of M. Cloëz‡, from which it appears that this chemist has interpreted my silence as a tacit admission of defeat; he rejects the formulæ which I have given for the diatomic derivatives of phenylamine and ethylamine, and blames me for having continued my researches on the diatomic bases without having previously replied to his observations.

Under these circumstances I have been compelled to resume the investigation of the action of dibromide of ethylene upon ammonia, and to reply, after nearly two years have elapsed without M. Cloëz's paper having been published, to the series of objections which this chemist has raised against the theory of the diatomic bases.

Since this continuation of my experiments throws considerable light upon this new class of compounds, I beg leave to submit them to the judgment of the Royal Society.

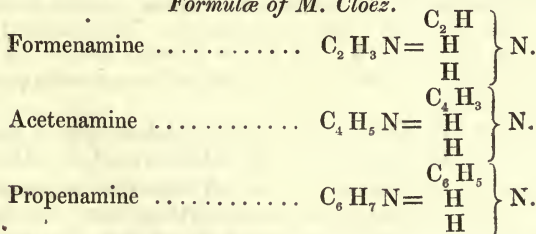
In order to render more intelligible the line of argument which M. Cloëz has brought forward against the diatomic notions, it will be useful to recapitulate in two words the subject of our controversy.

M. Cloëz admits that in the action of dibromide of ethylene upon ammonia, the molecule of ethylene splits into radicals belonging to three distinct groups, viz. the formic, acetic, and propionic series; these radicals acting upon *one* molecule of ammonia, in which each of them replaces one equivalent of hydrogen, give rise to the formation of three *primary monamines*, viz. Formenamine, Acetenamine, and Propenamine.

According to the view which I defend, the molecule of ethylene remains intact in the reaction, acting upon *two* molecules of ammonia in which 2, 4, or 6 equivalents of hydrogen are replaced respectively by 1, 2, or 3 diatomic molecules of ethylene; the dibromide of ethylene gives rise to the formation of three diamines belonging to the same family, a primary, a secondary, and a tertiary diamine.

Expressed in formulæ the two views may thus be represented:—

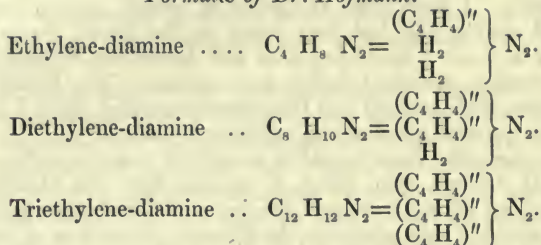
Formulæ of M. Cloëz.



* Phil. Mag. vol. xvii. pp. 66, 133; xviii. p. 148.

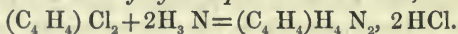
† Ibid. vol. xix. p. 232.

‡ L'Institut, 1859, p. 233.

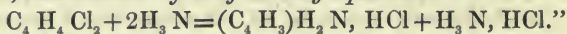
Formulæ of Dr. Hofmann.

It was by careful examination of the physical properties of the bases under consideration, and more especially by the absence of simple equations capable of explaining the formation of the first and of the third terms of the series, that I had first been led to doubt the correctness of M. Cloëz's formulæ; but I would not have expressed this doubt, if, on repeating the analysis of the first base, of formenamine, the slightest doubt on the subject had remained in my mind. I did not at the time investigate the two other bases, and I limited myself to stating that the constitution of these bodies would probably be found analogous to that which I had experimentally established for the first term of the series.

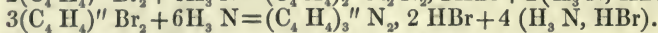
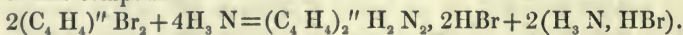
Let us now examine the objections which M. Cloëz has brought forward against my argument. "*According to the hypothesis of M. Hofmann,*" says he, "*the action of ammonia on the chlorinated and brominated hydrocarbons cannot give rise to the formation of chloride or bromide of ammonium; the reaction consists simply in a combination of the two substances, without the separation of a third compound: it is a case of symmorphosis or addition,*



Experiment proves, however, that the reaction involves the elimination of hydrochloric acid and the fixation of the elements of amidogen: apomorphosis and symmorphosis are accomplished side by side, as indicated by the following equation:—



M. Cloëz would be perfectly right if during the reaction no other base were formed except the first one. But he forgets altogether that in the process under examination—exactly as in the mutual reaction between bromide of ethyle and ammonia—several other bases of more advanced substitution are produced. The equations which I give for the formation of these bodies likewise involve the elimination of bromide of ammonium, and in fact of considerable quantities of this compound.



The bromide of ammonium then, which separates in considerable quantity in the action of dibromide of ethylene upon ammonia, belongs to the second and third portions of the reaction; it has nothing whatever to do with the formation of the first base. M. Cloëz, as I

have pointed out, does not admit the simple equation which I have given for the formation of this body; he denies that it is simply formed by the union of the two compounds reacting upon each other. According to his opinion it is produced in a secondary reaction, occasioned by the intervention of heat. My experiments do not confirm this opinion. A mixture of dibromide of ethylene and alcoholic ammonia allowed to stand for some time at the ordinary temperature, deposited a quantity of crystals, from which I was enabled to extract, without distillation, simply by successive crystallizations, absolutely pure salt of ethylene-diamine, as proved by the analysis of the bromide, the chloride, and the platinum-salt.

In discussing the numbers which I have obtained in analysing the hydrate and the hydrochlorate of the first base, M. Cloëz quotes the results on which he founds his own formula. A glance at these figures will show unmistakeably that they agree much better with my formula than with the one which he defends. The following are the analytical details of our analyses, together with the theoretical values required by each formula:—

	Formula of M. Cloëz.	Analysis of M. Cloëz.	Formula of Dr. Hofmann.	Analysis of Dr. Hofmann.
Carbon . . .	31·58	31·12	30·76	30·67
Hydrogen . .	10·52	12·77	12·82	12·97

Every experimentalist has incontestably the first right of interpreting his analytical results; knowing, as he does, his methods, he will do it generally much better than any other person. In the case before us, however, I believe very few chemists would have interpreted the results of analysis as M. Cloëz has done. As far as I am concerned, I would always prefer to admit having lost 0·2 per cent. of hydrogen, to calculating a formula requiring 2·25 per cent. of hydrogen less than had been obtained by experiment. I would prefer this especially in analysing a substance like ethylene-diamine, attracting carbonic acid with the utmost avidity—a trace of which would very appreciably lower the experimental hydrogen—and containing so high a percentage of hydrogen, that the presence even of a small quantity of water would produce a somewhat similar effect.

The results which M. Cloëz has obtained in the analysis of the hydrochlorate are not less in favour of my views. He finds 1·28 per cent. of hydrogen more than required by his formula, whilst admitting my theory, he would not have lost more than 0·13 per cent.

I have since examined several other salts of ethylene-diamine, and the results fully confirm the conclusions drawn from my former analyses. It would be useless to quote these additional experiments, but I will mention the characteristic numbers furnished by the analysis of the anhydrous base, since the diminution of the equivalent exhibits in a more striking manner the differences between the theoretical values of the two formulæ. Ethylene-diamine retains the water with the greatest energy, and it is in fact only by protracted

contact with metallic sodium that it is possible to obtain this body in the anhydrous condition. I give the numbers obtained by combustion, side by side with the theoretical values of the two formulæ:

	Formula of M. Cloëz, $C_2 H_3 N$.	Formula of Dr. Hofmann, $C_4 H_8 N_2$.	Analysis.
Carbon	41·37	40·00	40·13
Hydrogen	10·34	13·33	13·31

These numbers require no commentary.

It is not, however, in the results of analysis that M. Cloëz finds the chief support of his views; he quotes an observation which at the first glance appears fatal to the diatomic notions.

"But there is," continues M. Cloëz, *"a capital fact (un fait capital) which completely settles the question at issue: this is the vapour-density of the free base."*

This density has been found by experiment to be 1·42.

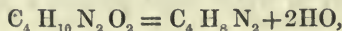
"The theoretical density calculated for my formula, referred to 4 volumes, is 1·315; the modified formula of M. Hofmann, likewise referred to 4 volumes, gives the theoretical density of 2·699."

"These results appear to me decisive, and I do not hesitate to maintain the formula of the new series of bases, of which I first pointed out the formation."

I entirely agree with M. Cloëz as to the importance of the determination of the vapour-densities, but I certainly arrive at a very different interpretation of his result.

In repeating the experiment of this able chemist, I have arrived, as might have been expected, at exactly the same number. But this number refers to the *hydrated base*, and it is easily seen that the hydrated molecule, when in the state of vapour, must occupy 8 volumes. In calculating the theoretical density corresponding to the diatomic formula when referred to 8 volumes, we arrive at the number 1·35, which coincides in fact with the number obtained by experiment.

It is obvious that under the influence of heat the hydrated base splits into anhydrous base (4 volumes) and water (4 volumes),



and that, instead of taking the vapour-density of the intact hydrated molecule, M. Cloëz has determined the density of a mixture of anhydrous base and water, which on cooling combined again, reproducing the hydrated compound. And here I must recall the observations of several chemists, especially those of M. Bineau, of M. Kekulé, and of M. H. Saint-Claire Deville, each of whom has had the opportunity of explaining the anomalous vapour-densities in the transitory decomposition of the compounds submitted to experiment; and I would quote particularly a note by Professor Kopp*, in which this distinguished physicist has treated the question of anomalous vapour-densities in a general manner.

In the case before us, there is a very simple experiment, calculated

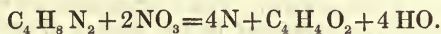
* Ann. der Chem. und Pharm. cv. 390.

to remove all hypothesis from the above explanation,—the determination of the vapour-density of the *anhydrous base*.

The experiment made with a substance the purity of which had previously been proved by analysis, led to the number 2·00, which indeed absolutely coincides with the theoretical density of the diatomic formula $C_4H_8N_2$ referred to 4 volumes. This theoretical density is 2·07, whilst the formula of M. Cloëz, likewise referred to 4 volumes, requires the theoretical density of 1·00.

The molecule of *ethylene-diamine* (formenamine) then, like those of all other well-examined organic compounds, corresponds to 4 volumes of vapour; and the vapour-density of the base, far from militating against the molecular value which I assign to this body, furnishes on the contrary an additional and incontestable argument in its favour.

The preceding remarks are, I hope, sufficient to establish the formulæ of the diatomic ammonias upon a solid basis. I will therefore only briefly allude to some results which I have obtained in studying the products of decomposition of ethylene-diamine, and which are not less characteristic. Submitted to the action of nitrous acid, this base is decomposed with evolution of nitrogen; in the first stage of the reaction an indifferent crystalline body is produced, and the final result of the process is a large quantity of pure oxalic acid. The nitrogen evolved during the transformation is accompanied by a very volatile liquid, the odour of which is somewhat similar to that of aldehyde. At the time when I made these experiments I really believed the liquid to be aldehyde, but since I failed in obtaining the crystalline compound with ammonia and in transforming it into acetic acid, I abstained from mentioning this reaction in my note to the Royal Society. I have now scarcely a doubt that the volatile liquid was the oxide of ethylene, isomeric with aldehyde, since discovered by M. Wurtz. The transformation would be



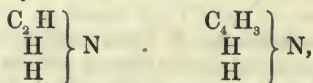
In preparing the ethylene-diamine for my experiments, I obtained as a secondary product a small quantity of the second base, which M. Cloëz has described as acetenamine, and for which I now propose the term diethylene-diamine. This base has exactly the same percentage composition, whether viewed as a diamine or considered as the monatomic acetenamine of M. Cloëz. The analysis of the base itself, and of some of its salts, fully confirms the results obtained by that chemist. But this base is no primary monamine; it does not contain the radical acetylene, C_4H_3 , as supposed by M. Cloëz; it is a secondary diamine containing two molecules of ethylene. Acetenamine, as conceived by M. Cloëz, should be formed by the action of chloride, bromide, and iodide of vinyle (C_4H_3Cl , C_4H_3Br , C_4H_3I) upon ammonia. These reactions do not furnish a trace of the base in question. But there is a more conclusive proof of the diatomic nature of this body, the evidence of which will not be contested by M. Cloëz,—this is the determination of the vapour-density. Experiment gave the number 2·7. The diatomic formula, $C_8H_{10}N_2$, referred

to 4 volumes of vapour, requires 2.9. According to the monatomic view, a vapour-density of 1.45 should have been found.

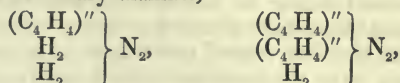
The preceding experiments, although fixing in a satisfactory manner the composition and the equivalents of the two diammonias, do not unveil their molecular constitution—their degree of substitution.

I have endeavoured to solve this problem by submitting them to the action of iodide of ethyle, a process which I first used for similar purposes, and which has since become of general application. This process, moreover, could not fail to furnish a final decision between the two theories.

In considering with M. Cloëz the two bases as primary monamines belonging respectively to the formic and to the acetic groups,



it is evident that each of them must be capable of absorbing successively 1, 2, or 3 equivalents of ethyle, and of yielding *three* ethylated bases, two volatile, and one fixed. On the contrary, if the bases were products of the successive substitution of the same molecule for the hydrogen of two equivalents of ammonia, if they were respectively a primary and a secondary diamine,



the first of the two must likewise give rise to the formation of *three* bases, whilst the second one would produce only *two*.

Experiment has verified this latter anticipation. In submitting ethylene-diamine to the alternate action of iodide of ethyle and oxide of silver, I have succeeded in obtaining two volatile ethylated bases, and a third one, which is fixed. These compounds are well defined; their composition was established by the analysis of their iodides or their platinum-salts. Represented as salts, these bases contain—

Salt of ethylene-diammonium $[(\text{C}_4\text{H}_4)'' \text{H}_6\text{N}_2]'' \text{I}_2$.

Salt of diethylated ethylene-diammonium . . $[(\text{C}_4\text{H}_4)'' (\text{C}_4\text{H}_5)_2\text{H}_4\text{N}_2]'' \text{I}_2$.

Salt of tetrethylated ethylene-diammonium $[(\text{C}_4\text{H}_4)'' (\text{C}_4\text{H}_5)_4\text{H}_2\text{N}_2]'' \text{I}_2$.

Salt of hexethylated ethylene-diammonium $[(\text{C}_4\text{H}_4)'' (\text{C}_4\text{H}_5)_6\text{N}_2]'' \text{I}_2$.

On repeating the same experiments with diethylene-diamine, perfectly analogous phenomena were observed, but the reaction yielded only one volatile base, which was immediately converted into a fixed base. Analysed in a similar manner, and represented as salts, these bases exhibit the following composition:—

Salt of diethylene-diammonium $[(\text{C}_4\text{H}_4)''_2 \text{H}_4\text{N}_2]'' \text{I}_2$.

Salt of diethylated diethylene-diammonium $[(\text{C}_4\text{H}_4)''_2 (\text{C}_4\text{H}_5)_2\text{H}_2\text{N}_2]'' \text{I}_2$.

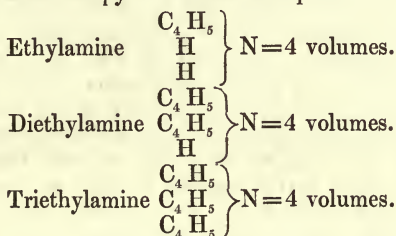
Salt of tetrethylated diethylene-diammonium $[(\text{C}_4\text{H}_4)''_2 (\text{C}_4\text{H}_5)_4\text{N}_2]'' \text{I}_2$.

The same result is accomplished, but in a shorter and more elegant manner, by substituting iodide of methyle for its ethylated homologue. Already, at an earlier period, I have shown that iodide of methyle has a remarkable tendency to yield the last product of substitution. Thus, on treating iodide of methyle with ammonia, the iodide of tetramethylammonium is alone obtained, together with a very large proportion of iodide of ammonium. The action of iodide of methyle with the ethylenated bases is perfectly analogous. The last product of substitution is formed at once in notable quantity, and may be purified by a simple crystallization. I have obtained in this manner, without being embarrassed by the intermediate compounds, the iodide of hexmethylated ethylene-diammonium and of tetramethylated diethylene-diammonium.

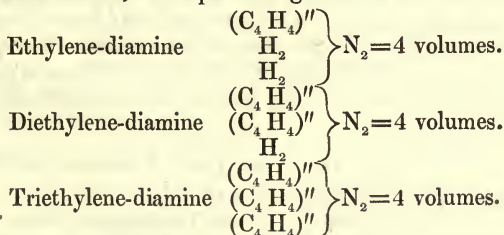
These results require no further explanation.

In the present state of science we rely upon a certain number of considerations which guide us in the construction of a chemical formula. These are,—the study of the origin of a body; analysis; observation of the physical properties, and especially of the boiling-point; the determination of the vapour-density; and lastly, the examination of its metamorphoses. I have endeavoured to look at the question under discussion from these several points of view; experiment has given invariably the same reply.

It follows from this controversy that the diatomic alcohols imitate the monatomic alcohols in their deportment with ammonia. Ethyl-alcohol produces, as is well known, three ethylated ammonias, the molecules of which occupy 4 volumes of vapour.



In a similar manner we find that glycol, the diatomic alcohol of ethylene, the discovery of which we owe to the remarkable labours of M. Wurtz, gives rise to three diatomic bases, corresponding to 2 molecules of ammonia, and representing likewise 4 volumes of vapour.



The two first terms of this series are the bases which M. Cloëz

discovered about six years ago, but the true nature of which he failed to recognize. To complete the series, it remains only to examine the third volatile base and the oxide of tetrethylene-diammonium.

The observations which I have the honour of submitting to the Royal Society coincide in every point with the first note upon this subject which I presented nearly two years ago. I have simply carried out somewhat more in detail the sketch traced in my former communication.

In conclusion I may state a fact which has also been observed by M. Cloëz, viz. that the action of dibromide of ethylene upon ammonia gives rise to the formation of bases not directly belonging to the series which we have discussed. In searching for the method of purifying the ethylene bases, I have been obliged to examine also the terms of the other group; but since these substances do not necessarily belong to this part of the inquiry, I omit for the present to enter more fully into their examination.

“On the Forces that produce the great Currents of the Air and of the Ocean.” By Thomas Hopkins, Esq.

In this paper the writer pointed out the fact that we have at present no satisfactory evidence in books of what are the immediate causes of the great currents of the air and of the ocean; and he maintained that the liberated heat of condensing vapour is the cause of these currents. He then proceeded to show that all the great winds terminate in comparative vacua created in particular localities where much vapour has been condensed; and contended that such vacua enable and cause heavier air to press and flow towards the parts which have been rendered light,—to re-establish the equilibrium of atmospheric pressure,—thus making heat the disturbing power in the aerial ocean, and leaving gravitation to act to restore an equilibrium. The great primary currents of the ocean were also described, and they were shown to be so situated as to be under the influence of the principal winds, which, in their passage over the waters, press on them, and force them forward as currents. These currents were maintained to be of a velocity, extent, and depth proportioned to the strength and continuity of the wind, showing that the pressure of the air on the water, whilst moving over it, is capable of producing the movement which takes place. When, however, water is put into motion, it may be obstructed by land, and turned from its direct course, and in that way be made to form secondary currents. But it was contended that heat of vapour, set free in the atmosphere, is the force which disturbs the equilibrium of pressure, and either directly or indirectly produces all the great continuous movements that take place both in the atmosphere and the ocean.

Jan. 19.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communication was read:—

“On the Interruption of the Voltaic Discharge in Vacuo by Magnetic Force.” By J. P. Gassiot, Esq., F.R.S.

The late Professor Daniell, in his Fifth Letter on Voltaic Combina-

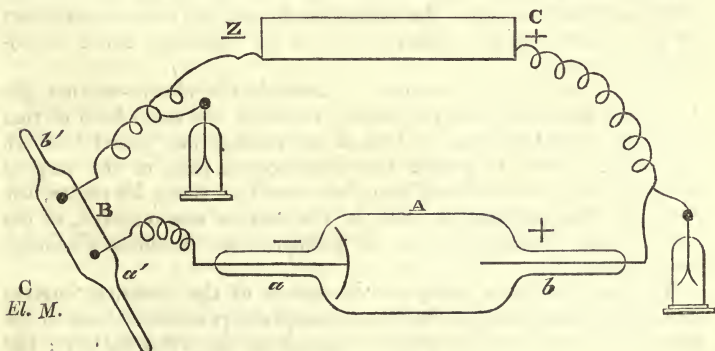
tions (Phil. Trans. 1839, part 1), describes some experiments made with seventy series of his constant battery, and states (page 93) "that the arc of flame between the electrodes was found to be attracted and repelled by the poles of a magnet, according as one or the other pole was held over or below it, as was first ascertained by Sir H. Davy; and the repulsion was at times so great as to extinguish the flame."

In the Philosophical Magazine of July 1858, Mr. Grove has described an experiment made by him with one of my vacuum-tubes, 2 feet 9 inches long, in which he ascertained that the discharge of a Ruhmkorff's induction coil could be stopped by bringing a magnet near the positive terminal wire, but that this effect was not obtained when the magnet was made to approach the negative. The mercurial vacuum-tube in which Mr. Grove observed this phenomenon was unfortunately shortly afterwards broken; and although Mr. Grove and myself have repeatedly endeavoured to obtain the same result in similar and in other vacuum-tubes (and since that period I have experimented with upwards of two hundred), all our efforts have been hitherto unsuccessful.

The experiments I am now about to describe were made with two carbonic acid vacuum-tubes, the vacua being obtained in the same manner as described by me in the Philosophical Transactions, part 1, 1859*.

A, in the annexed figure, represents a glass tubular vessel (No. 146), 24 inches long and 6 inches diameter in its wide part; at one end, attached to the platinum wire (*a*), is a concave copper plate 4 inches diameter, at the other end is a brass wire attached to the platinum wire (*b*). B represents a glass tube (196) 5 inches long (in its wide part), in which two small balls of gas-retort coke are attached to the platinum wires *a'* and *b'*, and are placed about 3 inches apart, all the platinum wires being hermetically sealed in the glass. In A the potash is placed in the vessel between the electrodes; in B it is placed in the further part of the tube, beyond one of the wires.

Battery.



* The carbon-balls do not in these experiments affect the results described, as I have obtained the same in a tube of the same dimension with brass wires.

An electro-magnet is placed at C, and is constructed so as to allow the two helices to be separated ; and by these means the larger vessel can, if required, be placed between them, and any portion of the luminous discharge may be thus exposed to any part of the magnetic field.

When the terminals of an excited induction coil are attached to the wires of either of the above vacuum-tubes A or B, luminous discharges are obtained, the negative wire ball or plate being covered with a luminous cloud-like glow extending towards the positive ; but the stratifications are not developed, except by the magnet, and these become more clearly defined as the magnet is caused to approach, or as the power is increased, when they are deflected according to the direction of the discharge, or of the polarity of the magnet. But with the induction coil, no matter how I reduced the intensity of the discharge, or varied that of the electro-magnet, in no instance could I produce in these, or in any of my vacuum-tubes, a similar result to that which Mr. Grove obtained in the vacuum-tube so unfortunately broken ; the experiment evidently requires a certain balance of power between the electric discharge and that of the magnet, and this I had hitherto been unsuccessful in obtaining.

I next experimented with my water-battery (Phil. Trans. 1844, and Proceedings, 26 May, 1859), which I have recently had carefully cleaned and recharged with rain-water ; the luminous discharge in both the vacua A and B was obtained with less than 1000 series, and this discharge, as well as that from the full series of the battery of 3520 cells, was under certain conditions, hereafter described, entirely destroyed or interrupted by the power of the magnet.

At first the interruption or break in the luminous discharge appeared to be caused by the sudden action of the magnet, as if it were merely momentarily blown out, for the discharge recovered itself while it remained under the influence of the magnet—the luminous discharge under this condition gradually reappearing stratified and strongly deflected ; but I subsequently ascertained that, by carefully adjusting the intensity of the battery discharge, and the force or power of the electro-magnet, this recovery in the discharge could be entirely prevented.

On approaching the vacuum A towards the electro-magnet, the luminous discharge from the battery assumed the same form as that from the induction coil ; but when the vacuum was placed between the helices, so as to permit the armatures or poles of the magnet to touch one or each side of the glass vessel at about its centre, the discharge disappeared ; as soon as the magnet was removed, or the vacuum-tube withdrawn from its influence, the luminous discharge was reproduced.

To test whether a complete disruption of the electrical current had taken place, two gold-leaf electroscopes were attached, one to the zinc and the other to the copper terminal of the water-battery ; the leaves diverged with considerable energy ; connection was then made from the electroscopes to the wires of the vacuum-tube ; the luminous

discharge became visible, and the leaves of both electroscopes partially collapsed; the vacuum-tube was then placed as before, between the armatures of the electro-magnet, and immediately the magnet was excited, the luminous discharge disappeared, and the leaves of the electroscopes diverged to their original maximum extent, thus proving the disruption to be complete.

If the smaller tube B is placed across both poles of the magnet, the luminous discharge at its centre assumes the appearance of being *nearly* separated into two parts, each part showing a tendency to rotate round the pole of the magnet on which it is placed, the one in an opposite direction to the other. I endeavoured to obtain a disruption of the battery discharge when in this state, and possibly with a more powerful electro-magnet this experiment would succeed; but although I reduced the intensity of the battery discharge and increased the power of my electro-magnet, I could not in this manner obtain an actual discontinuity of the battery discharge; but when the same vacuum-tube was placed in a longitudinal or equatorial position between the poles, or even approached them within three or four inches in that direction, an immediate interruption of the discharge took place.

When both vacuum-tubes are placed in the battery circuit, the interruption can be shown in a very striking manner: the general arrangement of the apparatus represented in the figure shows how this experiment is made. A is fixed on a wooden support. One wire (*b*) is attached to the copper terminal of the battery, the other wire (*a*) being connected to one of the wires in B, which is held by the hand, the other wire (*b'*) being connected with the zinc terminal of the battery, gold-leaf electroscopes being placed as before. In this manner all the apparatus is fixed except B, which being held by the hand, and the connecting wires being flexible, can be placed in any required position.

As long as the vacuums are at a sufficient distance from the action of the magnet, the luminous discharge is visible in both, and the leaves of the electroscopes partially collapse; but immediately the discharge in B is placed in the position described in the previous experiment, between the poles of the magnet, the discharges in *both* vacua instantly disappear, and the leaves of the electroscopes diverge to their original maximum.

The actual position of what is termed the magnetical field, around and between the poles of a magnet, has been generally delineated by means of iron filings placed between the poles on a sheet of paper. Assuming the lines in which these particles arrange themselves to represent the direction of the power of the magnet, or the magnetic field, they also explain the actual position through which the vacuum-tube should be placed to obtain the preceding result, and in this manner to show by experiment that a voltaic discharge which has sufficient intensity to pass through a space of upwards of 6 inches in attenuated carbonic acid gas is not only interrupted, but absolutely and entirely arrested by magnetic force.

Postscript.—In repeating the experiments with Dr. Tyndall in my

laboratory, the disruption of the luminous discharge *in vacuo* from 400 cells of the nitric-acid battery was obtained : some most beautiful and striking results were obtained from the same battery on the 16th inst., on repeating the experiment in the Theatre of the Royal Institution, with its large electro-magnet, Dr. Faraday and Dr. Tyndall being present.

The large receiver (146) was placed between the poles of the electro-magnet, the lines of force going through it; electrodes equatorial. The stratified discharge was extinguished. Subsequently, through the sinking of the battery, or some other cause, the stratifications disappeared, and the luminous glow which filled the entire tube remained. On now exciting the magnet with a battery of ten cells, effulgent strata were drawn out from the positive pole, and passing along the upper or under surface of the receiver, according to the direction of the current. On making the circuit of the magnet, and breaking it immediately, the luminous strata rushed from the positive and then retreated, cloud following cloud with a deliberate motion, and appearing as if swallowed by the positive electrode.

The amount of electricity which passed appeared materially increased on exciting the magnet; once the discharge was so intense as to fuse half an inch of the positive terminal.

After this had occurred, the discharge no longer passed as before when the terminals of the battery were connected with it; but on connecting the positive end of the battery with the gas-pipes of the building, the discharge passed.

The discharge could also be extinguished by the magnet; and the time necessary to accomplish this, furnished a beautiful indication of the gradual rise and reduction in the power of the electro-magnet.

ROYAL INSTITUTION OF GREAT BRITAIN.

March 30, 1860.—“On Acids and Salts.” By William Odling, Esq., M.B., F.R.S.

It is natural to inquire whether the doctrines of series and substitutions, which are essential for the association of organic products, may not throw some additional light upon the simpler compounds of mineral chemistry, when viewed as unitary molecules; and particularly upon the relations and properties of the mineral acids and their salts, which have hitherto constituted the strongholds of the electro-chemical, or binary, theory of combination.

The doctrine of series affirms that chemical compounds may be arranged in series, the successive members of each of which differ from one another in composition by a common increment, and are associated with one another by a certain relation of properties, the exact nature of the relation varying with the nature of the increment.

The doctrine of substitutions affirms that, in very many chemical compounds, one or more atoms may be displaced by some other atoms or groupings, and that the new bodies, resulting from this displacement, correspond in constitution with the normal bodies

from which they were derived. The doctrine of substitutions affords great assistance to the doctrine of series; for when, as frequently happens, a gap exists in any series, that gap can almost always be filled up by a substitution-representative of the missing body.

(α .) There are four acid compounds of hydrogen, two volumes of each of which contain one volume of hydrogen, namely:

HF	Fluorhydric acid.
HCl	Chlorhydric acid.
HBr	Bromhydric acid.
HI	Iodhydric acid.

When two volumes of chlorhydric acid, for instance, are acted upon by a red-hot iron wire, the chlorine is absorbed by the iron, and one volume of hydrogen gas liberated. The two volumes of chlorhydric acid yield one volume of hydrogen, or the original bulk of gas is reduced to one-half by the absorption of its chlorine. The above four acids may be looked upon as substitution-representatives, one of another.

Chlorhydric acid yields the following series of oxides, convertible into each other by mutual metamorphosis:—

HCl	Chlorhydric acid.
HClO	Hypochlorous acid.
HClO ²	Chlorous acid.
HClO ³	Chloric acid.
HClO ⁴	Perchloric acid.

When chlorhydric acid, HCl, is oxidated by permanganic acid, hypochlorous acid, HClO, is produced; and, conversely, chlorhydric acid may be reproduced by the deoxidation of hypochlorous acid. Hypochlorous acid, when heated, breaks up into chloric acid, HClO³, and other products. When chloric acid is deoxidated by nitrous acid, it becomes chlorous acid, HClO²; and when oxidated at the positive pole of a galvanic battery, it becomes perchloric acid, HClO⁴. Here then is a series of associated acids, expressed as unitary molecules, by the simplest possible formulæ, and arranged in a series, the successive members of which differ from one another in composition by an increment of one atom, or volume, of oxygen.

(β .) There are four other binary compounds of hydrogen, two volumes of each of which, however, contain two volumes of hydrogen, namely:—

H ² O	Water.
H ² S	Sulphydic acid.
H ² Se	Selenhydric acid.
H ² T	Tellurhydric acid.

A given volume of any one of these gases or vapours contains exactly twice the quantity of hydrogen that the same volume of any one of the first class of gases contains. When two volumes of sulphydic acid, for instance, are acted upon by a red-hot iron wire, the sulphur is absorbed by the iron, and two volumes of hydrogen gas are liberated. The two volumes of sulphydic acid yield two volumes of hydrogen, or the abstraction of the sulphur produces no alteration in the bulk of gas. The bihydric character of water, moreover, is well shown

by the experiment of its electrolytic decomposition, in which two volumes of hydrogen are produced for every one volume of oxygen.

In the sulphur series of oxygen acids we have two gaps, which, however, can be filled up by the chloro-representatives of the missing bodies, thus :

H^2S	Sulphydic acid.	Cl^2S .
H^2SO	Wanting.	Cl^2SO .
H^2SO^2	Wanting.	Cl^2SO^2 .
H^2SO^3	Sulphurous acid.	
H^2SO^4	Sulphuric acid.	

The compounds Cl^2SO and Cl^2SO^2 are obtainable from the chloro-representative of sulphydic acid, Cl^2S , by successive oxidation. The first product actually afforded by the oxidation of sulphydic acid is sulphurous acid, H^2SO^3 , which is produced by the combustion of sulphydic acid in air or oxygen. Conversely, sulphydic acid may be obtained by deoxidating sulphurous acid with nascent hydrogen. Sulphuric acid, H^2SO^4 , results from the oxidation of sulphurous acid, and by deoxidation can reproduce that body, as in the ordinary process for the preparation of sulphurous acid. Here then, including the chloro-representatives, is a second series of acids associated with one another by a common increment of composition, and by mutual metamorphosis.

Sulphuric acid, H^2SO^4 , is the representative on the sulphur series, of perchloric acid, HClO^4 , on the chlorine series. Each contains one atom of the radical which gives the special character to the acid, in the one case chlorine, in the other sulphur. Each contains also four atoms, or volumes, of oxygen ; but whereas perchloric acid contains only one atom, or volume, of hydrogen, sulphuric acid contains two atoms, or two volumes. And this difference in composition leads to a marked difference in the properties of the two acids. Perchloric acid, HClO^4 , has only one atom of hydrogen that can be replaced. Hence it forms only one description of salt, such, for instance, as perchlorate of potassium, KClO^4 , and only one description of ether, such, for instance, as perchloric ether, EtClO^4 . But sulphuric acid has two hydrogen atoms that can be replaced. Hence it can form acid salts, neutral salts, double salts, acid ethers, neutral ethers, double ethers, and saline ethers, as shown in the Table.

H^2SO^4	Sulphuric acid.
KHSO^4	Acid sulphate of potassium.
K^2SO^4	Neutral sulphate of potassium.
KNiSO^4	Potassio-sulphate of nickel.
EtHSO^4	Ethylo-sulphuric acid.
Et^2SO^4	Neutral sulphate of ethyle.
EtMeSO^4	Ethylo-sulphate of methyle.
EtKSO^4	Ethylo-sulphate of potassium.

This property of forming acid and double salts, and acid and double ethers, &c., indicates a fundamental difference in character between sulphuric and perchloric acids, a difference that is satisfactorily represented by the difference in their formulæ as here written down,

HClO^4 , and H^2SO^4 . Bibasic characters are manifested as decidedly by the sulphurous and sulphydric acids.

(γ .) There are four other binary compounds of hydrogen, two volumes of each of which, however, contain three volumes of hydrogen, namely :

H^3N Ammonia.
 H^3P Phosphamine.
 H^3As Arsenamine.
 H^3Sb Stibamine.

When the two volumes of phosphamine, for instance, are acted upon by a red hot iron-wire, the phosphorus is absorbed by the iron, and three volumes of hydrogen gas are liberated. Two volumes of chlorhydric acid yield one volume of hydrogen; two volumes of sulphydric acid yield two volumes of hydrogen, while two volumes of phosphamine yield three volumes of hydrogen; and this is a most important distinction between the three classes of hydrides to which these three gases respectively belong. Again, two volumes of gaseous ammonia, when decomposed by the Ruhmkorff spark, become converted into three volumes of hydrogen and one volume of nitrogen; or the original bulk of the ammonia becomes doubled.

In the phosphorus series of oxygen acids there is but one gap, and this can be filled up by the chlorine- or the ethyl-representative of the missing body.

H^3P	Phosphamine.	Cl^3P	Et^3P
H^3PO	Wanting.	Cl^3PO	Et^3PO
H^3PO^2	Hypophosphorous acid.		
H^3PO^3	Phosphorous acid.		
H^3PO^4	Phosphoric acid.		

Brodie has ascertained that oxychloride of phosphorus, Cl^3PO , may be obtained directly by passing oxygen gas through boiling terchloride of phosphorus, or trichloro-phosphamine, Cl^3P . The union of triethylphosphine, Et^3P , with oxygen, to form the oxide of triethylphosphine, Et^3PO , constituted one of Hofmann's earliest experiments on the phosphorus bases. Proceeding to the actual oxides of phosphamine, it is doubtful whether hypophosphorous acid, H^3PO^2 , has been obtained by the oxidation of phosphamine; but, on the other hand, phosphamine is readily obtainable by deoxidating hypophosphorous acid with nascent hydrogen; while by oxidating hypophosphorous acid, phosphorous and phosphoric acids are successively produced. Phosphorous acid, H^3PO^3 , results from the slow oxidation, and phosphoric acid, H^3PO^4 , from the rapid oxidation of phosphamine. Conversely, phosphamine may be obtained by the deoxidation of each of the two last-mentioned acids. Here again, then, is a series of naturally associated and mutually convertible bodies, represented by the simplest possible formulæ, by formulæ which do not express any speculative view whatever, but merely indicate the indisputable fact that these bodies, or their representatives, differ from one another in composition, by the successive increments of one, two, three, and four oxygen atoms.

Phosphoric acid, H^3PO^4 , is the representative in the phosphorus series, of sulphuric acid, H^2SO^4 , in the sulphur series, and of perchloric acid, HClO^4 , in the chlorine series; but whereas perchloric

acid contains only one atom of hydrogen, and can form only one class of salts and ethers ; whereas sulphuric acid contains only two atoms of hydrogen, and can form only two classes of salts and ethers ; phosphoric acid contains three atoms of hydrogen, and can form three classes of salts and ethers. One-third, two-thirds, or three-thirds of its hydrogen may be displaced by a metal or basic radical, or the hydrogen may be partly or wholly displaced by two or three different metals, or by two or three different radicals, or by a mixture of metals and radicals, thus :— EtKCuPO^4 , or $\text{H}(\text{NH}^4)\text{NaPO}^4$, &c.

(δ .) There is yet another primary hydride to be considered, namely, that of silicon, the siliciuretted hydrogen of Wöhler. The composition of this body has not been ascertained. It has been ascertained, however, that the substance from which it is obtained by the action of chlorhydric acid, is a silicide of magnesium, represented by the formula Mg^4Si , whence the formula of siliciuretted hydrogen is assumed to be H^4Si , analogous to that of marsh-gas, H^4C , a conclusion strongly confirmed by the composition of chloride of silicon, which is undoubtedly Cl^4Si , that is, a chloro-representative of siliciuretted hydrogen. Each primary hydride, hitherto considered, has yielded a remarkably stable acid, formed by the addition of four atoms of oxygen to the hydride ; and hydride of silicon ought to behave in the same manner, thus :—

Chlorhydric acid . . .	H Cl	H ClO^4	Perchloric acid.
Sulphydric acid . . .	H^2S	$\text{H}^2\text{S O}^4$	Sulphuric acid.
Phosphamine . . .	H^3P	$\text{H}^3\text{P O}^4$	Phosphoric acid.
Hydride of silicon ..	H^4Si	H^4SiO^4	Silicic acid.

Now whether or not H^4SiO^4 is the correct formula for silicic acid, it is certain that the great majority of simple and well-defined silicates may be referred to that type, as illustrated in the Table

Orthosilicates.

Et^4SiO^4	Silicic ether.	Gl^4SiO^4	Phenakite.
Li^4SiO^4	Silicate of lithium.	Ce^4SiO^4	Cerite.
$\text{Na}^2\text{H}^2\text{SiO}^4$	Silicate of sodium.	Fe^4SiO^4	Fayelite.
Ca^4SiO^4	Silicate of calcium.	$\text{Fe}^2\text{Mn}^2\text{SiO}^4$	Knebelite.
Mg^4SiO^4	Olivine, Chrysolite.	$\text{Cu}^2\text{H}^2\text{SiO}^4$	Diopase.
$\text{Ca}^2\text{Mg}^2\text{SiO}^4$	Batrachite.	$\text{Al}^3\text{CaSiO}^4$	Anorthite.
Zn^4SiO^4	Zinc glance.	$\text{Al}^3\text{MnSiO}^4$	Karpholite.

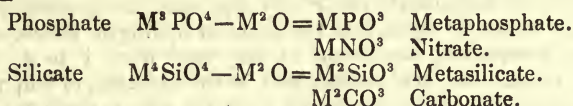
The next Table illustrates the general relations of the perchloric salts and ethers to their sulphuric, phosphoric, and silicic analogues. The existence of the silicated compounds corresponding to the formulæ in italics, has not yet been established.

Acids, Salts, and Ethers.

Acids, Salts, and Esters.

H Cl	H ClO ⁴	{ Na ClO ⁴ Et ClO ⁴				
H ² S	H ² SO ⁴	{ Na ² SO ⁴ Et ² SO ⁴	Na H SO ⁴ Et H SO ⁴			
H ³ P	H ³ PO ⁴	{ Na ³ PO ⁴ Et ³ PO ⁴	Na ² H PO ⁴ Et ² H PO ⁴	Na H ² PO ⁴ Et H ² PO ⁴		
H ⁴ Si	H ⁴ SiO ⁴	{ Na ⁴ SiO ⁴ Et ⁴ SiO ⁴	<i>Na³ H SiO⁴</i> <i>Et³ H SiO⁴</i>	Na ² H ² SiO ⁴ <i>Et² H² SiO⁴</i>	Na H ³ SiO ⁴ <i>Et H³ SiO⁴</i>	

Considering the relations of ammonia and phosphuretted hydrogen, H^3N and H^3P respectively, and the relations of marsh-gas and silicuretted hydrogen, H^4C and H^4Si respectively, there should exist nitrates and carbonates having the general formulæ M^3NO^4 and M^4CO^4 respectively, corresponding to ordinary phosphates and silicates having the general formulæ M^3PO^4 and M^4SiO^4 respectively. It is observable, however, that in addition to ordinary phosphates and silicates, there are other phosphates and silicates, known respectively as metaphosphates and metasilicates, which differ from the ordinary salts by the loss of an atom of base, and that it is these metasalts to which ordinary nitrates and carbonates correspond, thus:—



But chemists are acquainted with a considerable number of carbonates and nitrates, which may be called orthocarbonates and orthonitrates respectively, that do correspond in their formulæ with ordinary silicates and phosphates, as shown in the Table.

Orthocarbonates.

Ca^4CO^4	Dicarbonate of calcium.
Zn^4CO^4	Dicarbonate of zinc.
Mg^3HCO^4	Dicarbonate of magnesium.
Pb^4CO^4	Dicarbonate of lead.
Pb^3HCO^4	White lead.
Cu^4CO^4	Mysorine.
Cu^3HCO^4	Azurite.
$\text{Bi}''' \text{HCO}^4$	Dicarbonate of bismuth.

Orthonitrates.

Pb^3HNO^4	} of lead.
Pb^3NO^4	
Hg^3NO^4	} of mercury.
Hg^2HNO^4	
$\text{Bi}''' \text{NO}^4$	of bismuth.

The succeeding Tables present lists of the principal ter-oxygen and tetra-oxygen mineral acids. Some of these acids are known only through the medium of their metal- and ethyl-representatives.

Ter-Oxygen Acids.

Chloric	H Cl O^3
Bromic	H Br O^3
Iodic	H I O^3
Nitric	H N O^3
Metaphosphoric		H P O^3
Sulphurous	..	$\text{H}^2\text{S O}^3$
Selenious	$\text{H}^2\text{Se O}^3$
Tellurous	$\text{H}^2\text{T O}^3$
Carbonic	$\text{H}^2\text{C O}^3$
Metasilicic	$\text{H}^2\text{Si O}^3$

Tetra-Oxygen Acids.

Perchloric	H Cl O^4
Periodic	H I O^4
Permanganic	..	$\text{H Mn}^2\text{O}^4$
Sulphuric	$\text{H}^2\text{S O}^4$
Selenic	$\text{H}^2\text{Se O}^4$
Telluric	$\text{H}^2\text{T O}^4$
Oxalic	$\text{H}^2\text{C}^2\text{O}^4$
Molybdic	$\text{H}^2\text{Mo}^2\text{O}^4$
Vanadic	$\text{H}^2\text{V}^2\text{O}^4$
Tungstic	$\text{H}^2\text{W}^2\text{O}^4$

Tables (continued).

Ter-Oxygen Acids.			Tetra-Oxygen Acids.		
Titanic	$\text{H}^2 \text{Ti O}^3$	Chromic	$\text{H}^2 \text{Cr}^2 \text{O}^4$
Stannic	$\text{H}^2 \text{Sn O}^3$	Manganic	$\text{H}^2 \text{Mn}^2 \text{O}^4$
Vanadous	$\text{H}^2 \text{V}^2 \text{O}^3$	Ferric	$\text{H}^2 \text{Fe}^2 \text{O}^4$
Phosphorous	..	$\text{H}^3 \text{P O}^3$	Ortho-nitric	..	$\text{H}^3 \text{N O}^4$
Arsenious	$\text{H}^3 \text{As O}^3$	Phosphoric	..	$\text{H}^3 \text{P O}^4$
Antimonous	..	$\text{H}^3 \text{Sb O}^3$	Arsenic	$\text{H}^3 \text{As O}^4$
Bismuthous	..	$\text{H}^3 \text{Bi O}^3$	Antimonic	..	$\text{H}^3 \text{Sb O}^4$
Boracic	$\text{H}^3 \text{B O}^3$	Orthocarbonic		$\text{H}^4 \text{C O}^4$
Aluminous	..	$\text{H}^3 \text{Al}^2 \text{O}^3$	Silicic	$\text{H}^4 \text{Si O}^4$

Hence the formula $\text{H}^x \text{R}^y \text{O}^z$ will represent the general type for an acid, where H^x represents the atoms of hydrogen, which, save in carbon compounds, are found to vary only from 1 to 4; where R^y represents the acid radical, that is the chlorine, or sulphur, or phosphorus, or carbon, &c. which gives the special character to the acid, and which, save in carbon compounds, is usually confined to 1 or 2 elementary atoms; and where O^z represents the atoms of oxygen, which generally range from 0 to 4, but occasionally extend to higher numbers.

GEOLOGICAL SOCIETY.

[Continued from vol. xix. p. 468.]

April 18, 1860.—General Portlock, Vice-President, in the Chair.

The following communications were read:—

1. "On a Well-section at Bury Cross, near Gosport." By James Pilbrow, Esq. In a letter to the Assistant-Secretary.

This well, which was dug to a depth of 110, and bored 221 feet deeper, appears not to have penetrated the Bracklesham series of sands and clays, many of the characteristic fossils of which, obtained from the well, were exhibited by Mr. Pilbrow, together with specimens of the beds perforated. The yield of water in this well is very copious, certainly equal to 500,000 gallons at about 70 feet from the surface. When not pumped, the water rises to about 9 feet from the surface.

2. "On the presence of the London Clay in Norfolk, as proved by a boring at Yarmouth." By J. Prestwich, Esq., F.G.S.

In 1840 Sir E. Lacon and Co. commenced a well, for the supply of water to their brewery, and had a shaft dug to the depth of 22 feet, and then a boring made to the depth of 597 feet, entering the Chalk, but stopped by massive flints. The work was unsuccessful; but the specimens of the strata were carefully preserved: Mr. Prestwich and Mr. Rose lately examined them, and the following is Mr. Prestwich's opinion of the strata that they represent:—Blown sand and shingle, about 50 feet; recent estuarine deposits (with *Ostrea edulis*, *Cardium edule*, *Corbula Nucleus*, *Tellina Balthica*, *T. planata*, *Cyprina Islandica*, *Pecten opercularis*, *Mytilus*, and *Balanus*), 120 feet; London Clay, 310 feet; Woolwich and Reading series, 46 feet; Chalk, 57 feet.

This section is interesting as being illustrative of the estuary and

its filling up, and of the extension of London Clay and Lower Tertiary deposits to a more northerly point than had previously been ascertained.

3. "On some *Foraminifera* from the Upper Triassic Clays of Chellaston, near Derby." By T. Rupert Jones, Esq., F.G.S., and W. K. Parker, Esq., M. Micr. Soc.

Bluish-grey specimens of the mottled clay from the pits at Chellaston, 3 miles south of Derby, whence the alabaster is obtained, yielded abundance of minute *Foraminifera*, a few *Entomostraca* (*Cythere*), some Otolites, and spines and plates of small Echinoderms, together with fine siliceous sand and pyritous granules. Of the *Foraminifera* nearly one-half consist of a small variety of *Rotalia repanda*, namely *R. elegans*, D'Orb. The next most numerous group are the *Nodosarinæ*, including varieties of *Nodosaria*, *Dentalina*, *Marginulina*, *Vaginulina*, *Planularia*, *Fronicularia*, *Flabellina*, and *Cristellaria*. The genus next in numerical force is *Nubecularia*. *Polymorphina*, *Bulimina*, and *Lituola* are represented by a few individuals.

The authors stated that nearly all the varieties of the *Nodosarinæ* found at Chellaston are present in the Lias, in the clays of the Oolites, in the Gault, Chalk-marl, Chalk, some Tertiary deposits, and in some of the muds of the western Mediterranean and other seas; and the species of the other genera have also persisted to the present day. One of the Triassic forms was described as a new variety under the name of *Planularia pauperata*. It is found also in the Oolite and in the recent seas. The authors also entered at some length into the history of the genus *Nubecularia*, and described a new elongated variety under the name of *N. Tibia*. After describing the distribution of *Foraminifera* in many of the Mesozoic strata, and pointing out that *Nodosariæ*, *Textulariæ*, *Rotaliæ*, and some other *Foraminifera* occur in the palæozoic rocks, Messrs. Jones and Parker observed that altogether we have here some remarkable instances of the persistency of life-types among the lower animals. "Though the specific relationship of the palæozoic *Foraminifera* require further elucidation, we feel certain that the six genera represented in the Upper Keuper Clay of Chellaston by at least 30 varieties stand really in the place of ancestral representatives of certain existing *Foraminifera*,—that they put on their several subspecific features in accordance with the conditions of their place of growth, just as their posterity now do, and that, although we have in this instance met with only the minute forms of a 700 fathoms mud-bottom, yet elsewhere the contemporaneous fuller development of these specific types may be found by careful search in other and more shallow-water deposits of the Triassic period."

May 2, 1860.—L. Horner, Esq., President, in the Chair.

The following communications were read:—

1. "On the Physical Relations of the Reptiliferous Sandstone near Elgin." By the Rev. W. S. Symonds, F.G.S.

Referring to Sir R. Murchison's sections of the Elgin district, published in the Quart. Journ. Geol. Soc. No. 59, pp. 424 and 428,

which show a conformable sequence of strata from the Old Red Sandstone of Foths to the yellow sandstone and cornstone of Lossiemouth and Burgh Head, the author first stated that the siliceous marly rocks, or so-called "cornstones" of Glassgreen, Linksfeld, Spynie, Inverugie, and Lossiemouth are in reality very dissimilar to the cornstones of Foths and Cothall; and he then pointed out the improbability of the so-called cornstone of Glassgreen continuing to dip north-westwardly under the sandstone of the Quarry-wood Ridge, especially as near Linksfeld it is, at different spots, seen to dip away from that ridge. Evidence also of a break in the strata at the Bishop Mill quarries was brought forward to show that the sandstone beneath this "cornstone" (presumed to be the Reptiliferous sandstone) is probably faulted against the lower or Holoptychian sandstone, which latter towards Spynie was shown to be surmounted by the Reptiliferous sandstone, and this last conformably by a marly siliceous rock or so-called "cornstone."

Beyond Spynie Loch, northward, the author supposed that another fault had again brought up the sandstone with *Stagonolepis* and *Hyperodapedon* at Lossiemouth. Beyond this a cornstone-like rock is again seen to cover the sandstone.

The author then referred to the probable Liassic and Triassic character of the shales at Linksfeld, and dwelt upon the suggestion that had been offered as to the probability of the layer of boulder-clay beneath the shales having been due to the mass of shales being a portion of a cliff in the glacial period, and having then slipped from a higher level. Regarding these shales as having been removed merely by a slip from their original site, and as conformably overlying the calcareo-siliceous rock and sandstone beneath, Mr. Symonds expressed his belief that this sandstone, faulted against the Holoptychian sandstone at Quarry-wood, must be the Reptiliferous sandstone and of Triassic age. Lastly he remarked that the pebble-beds and sandstone, track-marked and rippled, of Burgh Head are far more like the Triassic conglomerates of England than like the Old Red rocks of Cothall and Foths.

2. "Notice of the Discovery of two Bone-caves in Northern Sicily." By Baron Anca de Mangalaviti. In a letter to Dr. Falconer, F.G.S.

One of the caves discovered by Baron Anca is at Monte Gallo, at the western extremity of the Bay of Palermo, the other near the village of Acque Dolci, at the foot of Monte San Fratello. These caves, especially the last, are very rich in bones, and contain large quantities of remains of Carnivora, including jaw-bones with molars and canines. Bones belonging to animals of the following genera have been met with:—*Hippopotamus*, *Elephas*, *Equus*, *Bos*, *Cervus*, *Canis*, *Ursus*, *Hyæna*, *Felis*, and some smaller Carnivores.

In these caves Baron Anca has found also a large quantity of flint implements, but only where remains of *Cervus* are abundant. Coprolites also, both of Carnivores and Herbivores, were met with.

The author has also met with teeth of Carnivora in the Grotta dell' Olivella.

VIII. *Intelligence and Miscellaneous Articles.*

CHEMICAL ANALYSIS OF TWO MINERAL PRODUCTS FORMED BY
SUBLIMATION IN THE ERUPTION OF VESUVIUS IN 1858. BY
M. CAPPA.

THESE two products, which by their physical properties appear to be *cotannite*, are of a yellow colour and not lustrous. The two specimens were marked A and B, and the results obtained were :—

A. Chlorine ; sulphuric, and traces of silicic acids ; lead in large quantity ; copper, and a small quantity of sodium.

B. Chlorine, copper, and lead.

A, from its physical and chemical properties, might be considered as oxychloride of lead mixed with small quantities of chlorides of copper and sodium, along with traces of sulphates and silicates. The compounds PbO , $PbCl$, and $2PbO$, $PbCl$ are well known. A seems to belong to the former species,—a supposition which appears more probable when it is remembered that oxychloride of lead is commercially obtained by treating oxide of lead with chloride of sodium and water. The hydrated body thus obtained becomes yellow by calcination. Probably the lead placed under the same circumstances in the presence of chloride of sodium and aqueous vapour, has given rise to the above compound.

B appears to be oxychloride of lead with a small quantity of chloride of copper.—*Comptes Rendus*, May 21, 1860.

ON THE FLUOZIRCONATES, AND ON THE FORMULA OF ZIRCONIA.

BY C. MARIGNAC.

The formula for zirconia generally adopted, Zr^2O^3 , was proposed by Berzelius, who was led to it by a comparison of the composition of two compounds which fluoride of zirconium forms with fluoride of potassium. The formula ZrO , assumed by some chemists, is quite indefensible. Neither it nor the formula Zr^2O^3 are in harmony with the properties of the body.

Recently Deville and Troost's researches* on the vapour-density of chloride of zirconium have led to the formula $ZrCl^2$. Zirconia, on this hypothesis, would be a binoxide allied to titanic, stannic, and silicic acids. The similarity of zirconium to silicium was pointed out by Berzelius, and he was only prevented from placing them together by the necessity of classifying the elements into metals and metalloids. The resemblance, too, of titanic acid and zirconia is so great, that it is almost impossible to separate them by analysis.

Marignac has undertaken a comparative study of the fluozirconates with a view of throwing light on the question. The material for the research was obtained by treating zirconia with hydrofluorate of fluoride of potassium at a red heat. A mixture of fluosilicate and fluozirconate of potash is obtained, from which the latter salt is readily obtained pure.

Fluoride of zirconium forms double salts with a great number of

† *Phil. Mag.* vol. xv. p. 459.

metallic fluorides; but it appears to officiate less powerfully as an acid than fluoride of silicon does. They are all decomposed by prolonged contact with the air.

The relation between the fluorine of the basic fluoride and that of the fluoride of zirconium is either—

I.	1 : 4
II.	2 : 4
III.	3 : 4
IV.	4 : 4

presenting a perfectly regular series, which precludes the possibility of assuming three atoms of fluorine in fluoride of zirconium.

The most stable of these compounds, as well as the most common, is the second. It may be taken as the type of the normal fluozirconate, $M Fl, Zr Fl^2$.

There is one crystallized compound which does not belong to the above series. It has the abnormal relation 5 : 8, which leads to the formula $5 Na Fl, 4 Zr Fl^2$. However singular this formula may appear, it is preferable to the more complicated one, $15 Na Fl, 8 Zr^2 Fl^3$, which the old formula of zirconia would require.

From the constitution of these salts, numerous instances of isomorphism between them and the fluosilicates, fluotitanates, and fluostannates might be expected. Such, however, is not the case. There is isomorphism only in the case of the zinc and nickel salts, which cannot be distinguished by their forms from the fluosilicates, &c.

The following is a list of the fluozirconates of which the crystalline forms have been determined. They are arranged in isomorphous groups :—

$K Fl, Zr Fl^2$ } $NH^4 Fl, Zr Fl^2$ }	Right rhomboidal prism.
$3 K Fl, 2 Zr Fl^2$ } $3 NH^4 Fl, 2 Zr Fl^2$ }	Regular octahedron.
$K Fl, 2 Zr Fl^2 + 2 Aq$ } $5 Na Fl, 4 Zr Fl^2$ }	.	Oblique rhomboidal prism.
$Mg Fl, Zr Fl^2 + 5 Aq$ } $Mn Fl, Zr Fl^2 + 5 Aq$ }	.	Id.
$Zn Fl, Zr Fl^2 + 6 Aq$ } $Ni Fl, Zr Fl^2 + 6 Aq$ }	.	Rhombohedral.
$K Fl, Ni Fl, 2 Zr Fl^2 + 8 Aq$..		Oblique rhomboidal prism.
$2 Mn Fl, Zr Fl^2 + 6 Aq$ } $2 Cd Fl, Zr Fl^2 + 6 Aq$ }	.	Id.
$2 Zn Fl, Zr Fl^2 + 12 Aq$ } $2 Ni Fl, Zr Fl^2 + 12 Aq$ }	.	Id.
$2 Cu Fl, 2 Zr Fl^2 + 12 Aq$ }	.	Id.
$3 Cu Fl, 2 Zr Fl^2 + 16 Aq$..		Id.

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IX. *Chemical Analysis by Spectrum-observations.*

By Professors KIRCHHOFF and BUNSEN*.

[With a Plate.]

IT is well known that certain substances possess the property of imparting definite colours to flames in which they are heated. When the coloured light thus produced is analysed by a prism, spectra exhibiting differently coloured bands or lines of light are seen. Upon the occurrence of these lines of light an entirely new method of qualitative chemical analysis can be based—a method which greatly enlarges the scope of chemical reactions, and points to the solution of problems hitherto unapproachable. In the present paper we shall confine ourselves to the application of this method to the detection of the metals of the alkalies and alkaline earths, and to the illustration of the value of the method in a series of examples.

The bright lines in the flame-spectrum are seen most plainly when the temperature of the flame is highest, and its illuminating power least. The laboratory-lamp described by one of us†, in which a mixture of air and gas burns, gives a flame of extremely high temperature and very slight luminosity, and is therefore especially well adapted for experiments upon the bright lines produced in the way described.

Plate II. represents the spectra given by a colourless flame in which chemically pure salts of potassium, sodium, lithium, strontium, calcium, barium are allowed to volatilize. The solar spectrum is added for the sake of comparison.

The potassium compound employed was obtained by igniting

* Communicated by Professor H. E. Roscoe.

† Poggendorff's *Annalen*, vol. c. p. 85; *Phil. Trans.* 1857, p. 377.
Phil. Mag. S. 4. Vol. 20. No. 131. Aug. 1860. H

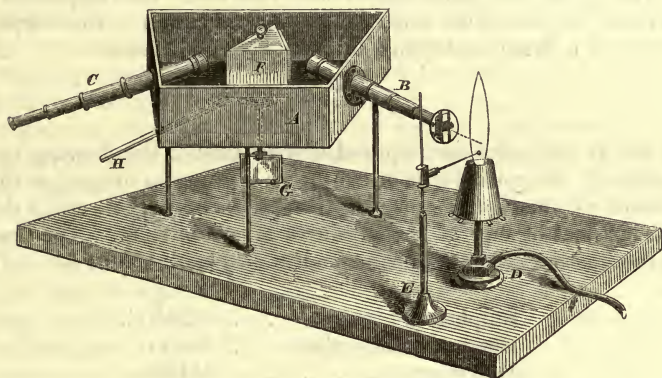
chlorate of potassium which had been previously recrystallized six or eight times. The chloride of sodium was prepared by neutralizing pure carbonate of sodium with hydrochloric acid and crystallizing the salt several times. The salt of lithium was purified by precipitation fourteen times with carbonate of ammonium. The purest specimen of marble which could be obtained, dissolved in hydrochloric acid, was the source of the calcium salt. From this solution the carbonate of calcium was thrown down in two portions by fractional precipitation with carbonate of ammonium, the latter half of the calcium salt being converted into the nitrate. The salt thus obtained was dissolved in absolute alcohol, and after evaporation of the alcohol, converted into the chloride by precipitation with carbonate of ammonium and solution in hydrochloric acid.

In order to obtain pure chloride of barium, the commercial salt was boiled out repeatedly with nearly absolute alcohol. The residual salt freed from alcohol was dissolved in water, and thrown down by fractional precipitation in two portions, of which the second only was dissolved in hydrochloric acid, and the salt still further purified by repeated crystallizations.

The pure chloride of strontium was prepared by crystallizing the commercial salt several times from alcohol, and by fractional precipitation of the salt with carbonate of ammonium, the second portion being dissolved in nitric acid, and the nitrate freed from the last traces of calcium salt by boiling with alcohol. The product thus purified was lastly thrown down with carbonate of ammonium, and the precipitate dissolved in hydrochloric acid. All these various purifications were conducted as much as possible in platinum vessels.

The apparatus which we have usually employed for our spectrum-observations is represented in the annexed woodcut. A consists of a box blackened on the inside, the bottom of which has the form of a trapezium, and rests on three feet; the two inclined sides of the box, which are placed at an angle of about 58° from each other, carry the two small telescopes B and C. The ocular lenses of the first telescope are removed, and in their place is inserted a plate, in which a slit made by two brass knife-edges is so arranged that it coincides with the focus of the object-glass. The gas-lamp D stands before the slit in a position such that the mantle of the flame is in a straight line with the axis of the telescope B. Somewhat lower than the point at which the axis of the tube produced meets the mantle, the end of a fine platinum wire bent round to a hook is placed in the flame. The platinum wire is supported in this position by a small holder, E, and on to the hook is melted a globule of the dried chloride which it is required to examine. Between the

object-glasses of the telescopes B and C is placed a hollow prism, F, filled with bisulphide of carbon, and having a refracting angle of 60° . The prism rests upon a brass plate moveable about a vertical axis. The axis carries on its lower part the mirror G, and above that the arm H, which serves as a handle



for turning the prism and mirror. A small telescope placed some way off is directed towards the mirror, and through this telescope an image of a horizontal scale, fixed at some distance from the mirror, is observed. By turning the prism round, every colour of the spectrum may be made to move past the vertical wire of the telescope C, and any required position in the spectrum thus brought to coincide with this vertical line. Each particular portion of the spectrum thus corresponds to a certain point on the scale. If the luminosity of the spectrum is very small, the wire of the telescope C may be illuminated by means of a lens, which throws a portion of the rays from a lamp through a small opening in the side of the tube of the telescope C.

We have compared the spectra represented on the Plate, which we have obtained from the pure chlorides, with those produced when the bromides, iodides, hydrated oxides, sulphates and carbonates of the several metals are brought into the following flames :—

Into the flame of sulphur.

„ „ bisulphide of carbon.

„ „ aqueous alcohol.

Into the non-luminous flame of coal-gas.

Into the flame of carbonic oxide.

„ „ hydrogen.

Into the oxyhydrogen flame.

As the result of these somewhat lengthy experiments, the details of which we here omit, it appears that the alteration of
H 2

the bodies with which the metals employed were combined, the variety in the nature of the chemical processes occurring in the several flames, and the wide differences of temperature which these flames exhibit, *produce no effect upon the position of the bright lines in the spectrum which are characteristic of each metal.*

The following considerations show how much the temperature of these various flames differ. An approximation to the temperature of a flame is obtained by help of the equation

$$t = \frac{\sum gw}{\sum ps},$$

in which t signifies the required temperature of the flame, g the weight of one constituent of substance burning in oxygen, w the heat of combustion of this constituent, p the weight, and s the specific heat of one of the products of combustion. The heat of combustion of the following bodies may be taken as—

Sulphur	2240° C.
Bisulphide of carbon . .	3400
Hydrogen	34462
Marsh-gas	13063
Elayle	11640
Ditetryle	11529
Carbonic oxide	2403

The specific heats under constant pressure were found by Regnault to be—

Sulphurous acid	= 0·1553
Carbonic acid	= 0·2164
Nitrogen	= 0·2440
Aqueous vapour	= 0·4750

Hence the temperatures of the flames are found to be—

The sulphur flame	1820° C.
The bisulphide of carbon flame . .	2195
The coal-gas flame*	2350
The carbonic oxide flame†	3042
The hydrogen flame in air‡	3259
The oxyhydrogen flame§	8061

It was found that the same metallic compound, placed in one of these flames, gives a more intense spectrum the higher the temperature of the flame. In the same flame, those of the compounds of a metal give the brightest spectra which are most volatile.

* *Ann. Chem. Pharm.* vol. cxi. p. 258.

† Bunsen's 'Gasometry,' p. 242.

‡ Ibid.

§ Ibid.

In order to prove still more conclusively that each of the above-mentioned metals always produces the same bright lines in the spectrum, we have compared the spectra represented in the Plate with those produced when the electric spark passes between electrodes made of these metals.

Small pieces of sodium, potassium, lithium, strontium, and calcium were fastened to fine platinum wires and melted two by two into glass tubes, so that the pieces of metal were separated by about 1 to 2 millims., and the platinum wires were melted through the sides of the glass tubes. Each of these tubes was placed in front of the spectrum-instrument, and by means of a Ruhmkorff's induction apparatus, sparks were allowed to pass between the pieces of metal inside the tube; the spectrum thus produced was then compared with that given by a gas-flame in which the chloride of the metal was brought. The flame was placed behind the glass tube. By alternately bringing the induction apparatus into and out of action, it was easy, without measuring, to convince ourselves that in the brilliant spectrum of the electric spark, the bright lines of the flame-spectrum were present in their right position. Besides these lines, other bright ones appeared in the electric-spark spectrum; some of these were produced by foreign metals present in the electrodes, others arose from nitrogen, which filled the tubes after the oxygen had combined with a portion of the electrodes*.

From these facts it appears certain that the appearance of the bright lines represented in the spectra on the Plate may be regarded as absolute proof of the presence of the particular metal. They serve as reactions by means of which these bodies may be recognized with more certainty, greater quickness, and in far smaller quantities than can be done by help of any other known analytical method.

The spectra drawn on the Plate represent those seen when the slit was of such a width that only the most conspicuous of the lines of the solar spectrum were visible, the magnifying power of the telescope C being a fourfold one, and the light of a moderate degree of intensity. These circumstances seem to us to be the most advantageous when it is required to make a chemical analysis by means of spectrum-observations. The

* On employing on one occasion with strontium-electrodes a tube filled with hydrogen instead of nitrogen, the stream of sparks changed rapidly into a continuous arc of light, whilst a grey pellicle covered the inside of the tube. The tube was opened under rock-oil, when it was found that it was empty, the hydrogen having disappeared. This gas appears, at the enormous temperature of the electric spark, to have decomposed the oxide of strontium which was not completely removed from the metal.

appearance of the spectrum can under other conditions vary considerably. If the purity of the spectrum be increased, many of those lines which appeared before as single ones are split up into several; thus the sodium line is divided into two separate lines. If the intensity of the light, on the other hand, be increased, new lines appear in several of the spectra represented in the Plate, and the relation of the brightness of the old ones becomes altered. In general an indistinct line becomes brighter upon increasing the illumination, more rapidly than does a brighter line, but not to such an extent that the indistinct line ever overtakes in intensity the brighter one. A good example of this is seen in the two lithium lines. We have only observed one exception to this rule, namely in the line $Ba\eta$, which by light of small intensity is scarcely visible, whilst $Ba\gamma$ appears plainly, but by light of greater intensity becomes more visible than the latter. This fact appears to us of importance, and we intend on a future occasion to examine this point in detail.

We now proceed to describe the peculiarities of the several spectra, the exact acquaintance with which is of practical importance, and to point out the advantages which this new method of chemical analysis possesses over the older processes.

Sodium.

The spectrum-reaction of sodium is the most delicate of all. The yellow line $Na\alpha$, the only one which appears in the sodium spectrum, is coincident with Fraunhofer's dark line D, and is remarkable for its exactly defined form, and for its extraordinary degree of brightness. If the temperature of the flame be very high, and the quantity of the substance employed very large, traces of a continuous spectrum are seen in the immediate neighbourhood of the line. In this case too, the weaker lines produced by other bodies, when near the sodium line, are discerned with difficulty, and are often first seen when the sodium reaction has almost subsided.

The reaction is most visible in the sodium salts of oxygen, chlorine, iodine, bromine, sulphuric acid, and carbonic acid. But even in the silicates, borates, phosphates, and other non-volatile salts, the reaction is always evident.

Swan* has already remarked upon the small quantity of sodium necessary to produce the yellow line.

The following experiment shows that the chemist possesses no reaction which in the slightest degree will bear comparison, as regards delicacy, with this spectrum-analytical determination of sodium. In a far corner of our experiment room, the capacity

* Trans. Roy. Soc. Edinb. vol. xxi. Part III. p. 411.

of which was about 60 cubic metres, we burnt a mixture of 3 milligrammes of chlorate of sodium with milk-sugar, whilst the non-luminous colourless flame of the lamp was observed through the slit of the telescope. Within a few minutes the flame, which gradually became pale yellow, gave a distinct sodium line, which, after lasting for ten minutes, entirely disappeared. From the weight of sodium salt burnt, and the capacity of the room, it is easy to calculate that in one part by weight of air there is suspended less than $\frac{1}{20,000,000}$ of a part of soda-smoke. As the reaction can be observed with all possible comfort in one second, and as in this time the quantity of air which is heated to ignition by the flame is found, from the rate of issue and from the composition of the gases of the flame, to be only about 50 cub. cent. or 0.0647 grm. of air, containing less than $\frac{1}{20,000,000}$ of sodium salt, it follows that the eye is able to detect with the greatest ease quantities of sodium salt less than $\frac{1}{3,000,000}$ of a milligramme in weight. With a reaction so delicate, it is easy to understand why a sodium reaction is almost always noticed in ignited atmospheric air. More than two-thirds of the earth's surface is covered with a solution of chloride of sodium, fine particles of which are continually being carried into the air by the action of the waves. These particles of sea-water cast thus into the atmosphere, evaporate, leaving almost inconceivably small residues, which, floating about, are almost always present in the air, and are rendered evident to our eyesight in the sunbeam. These minute particles perhaps serve to supply the smaller organized bodies with the salts which larger animals and plants obtain from the ground; in another point of view, however, the presence of this chloride of sodium in the air is of interest. If, as is scarcely doubtful at the present time, the explanation of the spread of contagious disease is to be sought for in some peculiar contact-action, it is possible that the presence of so antiseptic a substance as chloride of sodium, even in almost infinitely small quantities, may not be without influence upon such occurrences in the atmosphere. By means of daily and long-continued spectrum-observations, it would be easy to discover whether the alteration of intensity in the line $\text{Na } \alpha$ produced by the sodium in the air, have any connexion with the appearance and direction of march of an endemic disease.

The unexampled delicacy of the sodium reaction explains also the well-observed fact, that all bodies after a lengthened exposure to air show the sodium line when brought into a flame, and that it is only possible in a few salts to get rid of the last traces of the line $\text{Na } \alpha$, even after repeated crystallization from water which had only been in contact with platinum. A thin platinum

wire, freed from every trace of sodium salt by ignition, shows the reaction most visibly on allowing it to stand for a few hours in the air. In the same way the dust which settles from the air in a room, shows the bright line $\text{Na } \alpha$: to render this evident it is only necessary to knock a dusty book, for instance, at a distance of some feet from the flame, when a wonderfully bright flash of the yellow band is seen.

Lithium.

The luminous ignited vapour of the lithium compounds gives two sharply defined lines, the one a very weak yellow line, $\text{Li } \beta$, and the other a bright red line, $\text{Li } \alpha$. This reaction exceeds in certainty and delicacy all methods hitherto known in analytical chemistry. It is, however, not quite so sensitive as the sodium reaction, only, perhaps, because the eye is more adapted to distinguish yellow than red rays. When 9 milligrammes of carbonate of lithium mixed with excess of milk-sugar was burnt, the reaction was visible in a room of 60 cubic metre capacity. Hence, according to the method already explained, we find that the eye is capable of distinguishing with absolute certainty a quantity of carbonate of lithium less than $\frac{9}{10,000,000}$ of a milligramme in weight: 0.05 grm. of carbonate of lithium salt, burnt in the same room, was sufficient to enable the ignited air to show the red line $\text{Li } \alpha$ for an hour after the combustion had taken place.

The compounds of lithium with oxygen, iodine, bromine, and chlorine are the most suitable for this peculiar reaction; still the carbonate, sulphate, and even the phosphate give almost as distinct a reaction. Minerals containing lithium, such as triphylline, triphane, petalite, lepidolite, require only to be held in the flame in order to obtain the bright line $\text{Li } \alpha$ in the most satisfactory manner. In this way the presence of lithium in many feldspars can be directly detected, as, for instance, in the orthoclase from Baveno. The line is only seen for a few moments, directly after the mineral is brought into the flame. In the same way the mica from Altenberg and Penig was found to contain lithium, whereas micas from Miask, Ashaffenburg, Modum, Bengal, Pennsylvania, &c., were found to be free from this metal. In natural silicates which contain only small traces of lithium, this metal is not observed so readily. The examination is then best conducted as follows:—A small portion of the substance is digested and evaporated with hydrofluoric acid or fluoride of ammonium, the residue moistened with sulphuric acid and heated, the dry mass being dissolved in absolute alcohol. The alcoholic extract is then evaporated, the dry mass again dissolved in alcohol, and the extract allowed to evaporate on a shal-

low glass dish. The solid pellicle which remains is scraped off with a fine knife, and brought into the flame upon the thin platinum wire. For one experiment, $\frac{1}{10}$ of a milligramme is in general quite a sufficient quantity. Other compounds besides the silicates, in which small traces of lithium require to be detected, are transformed into sulphates by evaporation with sulphuric acid or otherwise, and then treated in the manner described.

In this way we arrive at the unexpected conclusion that lithium is most widely distributed throughout nature, occurring in almost all bodies. Lithium was easily detected in 40 cubic centimetres of the water of the Atlantic Ocean, collected in 41° 41' N. latitude, and 39° 14' W. longitude. Ashes of marine plants (kelp), driven by the Gulf-stream on the Scotch coasts, contain evident traces of this metal. All the orthoclase and quartz from the granite of the Odenwald which we have examined contain lithium. A very pure spring water from the granite in Schlierbach, on the west side of the valley of the Neckar, was found to contain lithium, whereas the water from the red sandstone which supplies the Heidelberg laboratory was shown to contain none of this metal. Mineral waters, in a litre of which lithium could hardly be detected according to the ordinary methods of analysis, gave plainly the line Li α , even if only a drop of the water on a platinum wire was brought into the flame*. All the ashes of plants growing in the Odenwald on a granite soil, as well as Russian and other potashes, contain lithium. Even in the ashes of tobacco, vine leaves, of the wood of the vine, and of grapes†, as well as in the ashes of the crops grown in the Rhine-plain near Wäghausel, Deidesheim, and Heidelberg, on a non-granitic soil, was lithium found. The milk of the animals fed upon these crops also contains this widely diffused metal‡.

It is scarcely necessary to say that a mixture of volatile sodium and lithium salts gives the reaction of lithium alongside that of sodium with a precision and distinctness which are hardly perceptibly diminished. The red lines of the former substance are still plainly seen when the bead contains $\frac{1}{1000}$ part of lithium salt, and when to the naked eye the yellow soda-flame appears untinged by the slightest trace of red. In consequence of the

* When liquids have to be brought into the flame, it is best to bend the end of the platinum wire, of the thickness of a horsehair, to a small ring, and to beat this ring flat. If a drop of liquid be brought into this ring, enough adheres to the wire for one experiment.

† In the manufactories of tartaric acid, the mother-liquors contain so much lithium salts that considerable quantities can thus be prepared.

‡ Dr. Folwarczny has been able to detect lithium in the ash of human blood and muscular tissue by help of the line Li α .

somewhat greater volatility of the lithium salt, the sodium reaction lasts longer than that of the other metal. In those cases, therefore, in which small quantities of lithium have to be detected in presence of large quantities of sodium salt, the bead must be brought into the flame whilst the observer is looking through the telescope. The lithium lines are often only seen during a few moments amongst the first products of the volatilization.

In the production of lithium salts on the large scale, in the proper choice of a raw material, and in the arrangement of suitable methods of separation, this spectrum-analysis affords most valuable aid. Thus it is only necessary to place a drop of mother-liquor from any mineral spring in the flame and to observe the spectrum produced, in order to show that in many of these waste products a rich and hitherto unheeded source of lithium salts exists. In the same way during the course of the preparation any loss of lithium in the collateral products and residues can be easily traced, and thus more convenient and economical methods of preparation may be found to replace those at present employed*.

Potassium.

The volatile potassium compounds give, when placed in the flame, a widely extended continuous spectrum, which contains only two characteristic lines, namely, one line, $Ka\alpha$, in the outermost red approaching the ultra-red rays, exactly coinciding with the dark line A of the solar spectrum, and a second line, $Ka\beta$, situated far in the violet rays towards the other end of the spectrum, and also identical with a particular dark line observed by Fraunhofer. A very indistinct line coinciding with Fraunhofer's line B, which, however, is only seen when the light is very intense, is not by any means so characteristic. The violet line is somewhat pale, but can be used almost as well as the red line for the detection of potassium. Owing to the position of these two lines, both situated near the limit at which our eyes cease to be sensitive to the rays, this reaction for potassium is not so delicate as the reaction for the two metals already mentioned. The reaction became visible in the air of our room when 1 gramme of chlorate of potassium mixed with milk-sugar was burnt. In this way, therefore, the

* We obtained by such an improved method from two jars (about 4 litres) of a mother-liquor from a mineral spring, which by evaporation with sulphuric acid gave 1 $\frac{1}{2}$ of residue, half an ounce of carbonate of lithium of the purity of the commercial, the cost of which is about 140 florins the pound. A great number of other mineral-spring mother-liquors which we examined showed a similar richness in compounds of lithium.

eye requires the presence of $\frac{1}{1000}$ of a milligramme of chlorate of potassium in order to detect the presence of potassium.

Caustic potash, and all compounds of potassium with volatile acids, give the reaction without exception. Potash silicates, and other non-volatile salts, on the other hand, only produce the reaction when the metal is present in very large quantities. It is only necessary, however, to melt the substance with a bead of carbonate of sodium, in order to detect potassium even when present in a very small quantity. The presence of the sodium does not in the least interfere with the reaction, and scarcely diminishes its delicacy. Orthoclase, sanidine, and adularia may in this way be easily distinguished from albite, oligoclase, Labradorite, and anorthite. In order to detect the smallest traces of potassium salt, the silicate requires only to be slightly ignited with a large excess of fluoride of ammonium on a platinum capsule, after which the residue is brought into the flame on a platinum wire. In this way it is found that almost every silicate contains potash. Salts of lithium diminish or influence the reaction as little as soda salts. Thus we need only to hold the end of a burnt cigar in the flame before the slit, in order at once to see the yellow line of sodium and the two red lines of potassium and lithium, this latter metal being scarcely ever absent in tobacco ash.

Strontium.

The spectra produced by the alkaline earths are by no means so simple as those produced by the alkalis. That of strontium is especially characterized by the absence of green bands. Eight lines in the strontium-spectrum are remarkable, namely, six red, one orange, and one blue line. The orange line, $\text{Sr } \alpha$, which appears close by the sodium line towards the red end of the spectrum, the two red lines, $\text{Sr } \beta$ and $\text{Sr } \gamma$, and lastly, the blue line, $\text{Sr } \delta$, are the most important strontium bands, both as regards their position and their intensity. For the purpose of examining the intensity of the reaction, we quickly heated an aqueous solution of chloride of strontium, of a known degree of concentration, in a platinum dish over a large flame until the water was evaporated and the basin became red-hot. The salt then began to decrepitate, and was thrown in microscopic particles out of the dish in the form of a white cloud carried up into the air. On weighing the residual quantity of salt, it was found that in this way 0.077 grm. of chloride of strontium had been mixed in the form of a fine dust with the air of the room, weighing 77000 grms. As soon as the air in the room was perfectly mixed, by rapidly moving an open umbrella, the characteristic lines of the strontium-spectrum were beautifully seen. According to this

experiment, a quantity of strontium may be thus detected equal to the $\frac{6}{100,000}$ part of a milligramme in weight.

The chlorine and the other haloid salts of strontium give the best reaction. The hydrated oxide and the carbonate of strontium give the action less vividly, the sulphate less distinctly, whilst the compounds of strontium with the non-volatile acids give either a very slight reaction or else none at all. Hence it is well first to bring the bead of substance alone into the flame, and then again after moistening with hydrochloric acid. If it be supposed that sulphuric acid is present in the bead, it must be held in the reducing part of the flame before it is moistened with hydrochloric acid, for the purpose of changing the sulphate into the sulphide, which is decomposed by hydrochloric acid. In order to detect strontium when combined with silicic, phosphoric, boracic, and other non-volatile acids, the following course of procedure gives the best results. Instead of fusing with carbonate of sodium in a platinum crucible, a conical spiral of platinum wire is employed; this spiral is heated to whiteness in the flame, and dipped whilst hot into finely powdered dried carbonate of sodium, which properly should contain so much water that a sufficient quantity adheres to the wire when it is once dipped into the salt. The fusion takes place in this spiral much more quickly than in a platinum crucible, as the mass of platinum requiring heating is small, and the flame comes into direct contact with the salt. As soon as the finely powdered mineral has been brought into the fused soda by means of a small platinum spatula, and the mass retained above the fusion point for a few minutes, the cooled mass has only to be turned upside down, and knocked on the porcelain plate of the lamp in order to obtain the salt in one coherent bead. The fused mass is covered by a piece of writing-paper, and then broken by pressing it with the blade of a steel spatula until the whole is in the state of a fine powder. The powder is collected to one spot on the edge of the plate, and carefully covered with hot water, which is allowed to flow backwards and forwards over it, so that, after decanting and rewashing the powder several times, all the soluble salts are extracted without losing any of the residue. If a solution of chloride of sodium be employed instead of water, the operation may be conducted still more rapidly. The insoluble salt contains the strontium as carbonate; and one or two tenths of a milligramme of the substance, brought on to the wire and moistened with hydrochloric acid, is sufficient to produce the most intense reaction. It is thus possible, without help of platinum crucible, mortar, evaporating basin, or funnel and filter, to fuse, powder, digest, and wash out the substance in the space of a few minutes.

The reactions of potassium and sodium are not influenced by the presence of strontium. Lithium also can be easily detected in presence of strontium when the proportion of the former metal is not very small. The lithium line $\text{Li } \alpha$ appears as an intensely red sharply defined band upon a less distinct red ground of the broad strontium band $\text{Sr } \beta$.

Calcium.

The spectrum produced by calcium is immediately distinguished from the four spectra already considered by the very characteristic bright green line $\text{Ca } \beta$. A second no less characteristic feature in the calcium spectrum is the intensely bright orange line $\text{Ca } \alpha$, lying considerably nearer to the red end of the spectrum than either the sodium line $\text{Na } \alpha$, or the orange band of strontium $\text{Sr } \alpha$. By burning a mixture consisting of chloride of calcium, chlorate of potassium, and milk-sugar, a white cloud is obtained which gives the reaction with as great a degree of delicacy as strontium salts do under similar circumstances. In this way we found that $\frac{6}{10,000,000}$ of a milligramme in weight of chloride of calcium can be detected with certainty. Only the volatile compounds of calcium give this reaction; the more volatile the salt the more distinct and delicate does the reaction become. The chloride, bromide, and iodide of calcium are in this respect the best compounds. Sulphate of calcium produces the spectrum, after it has become basic, very brightly and continuously. In the same way the reaction of the carbonate becomes more distinctly visible after the acid has been expelled.

Compounds of calcium with the non-volatile acids remain inactive in the flame; but if they are attacked by hydrochloric acid, the reaction may be easily obtained as follows:—A few milligrammes of finely powdered substance are brought on to the moistened flat platinum ring in the moderately hot portion of the flame, so that the powder is fritted, but not melted on to the wire; if a drop of hydrochloric acid be now allowed to fall into the ring so that the greater part of the acid remains hanging on to the wire, and if then the wire be brought into the hottest part of the flame, the drop evaporates in the spheroidal state without ebullition. The spectrum of the flame must be observed during this operation; and it will be noticed that at the moment when the last particles of liquid evaporate, a bright calcium spectrum appears. If the quantities of the metal present are very small, the characteristic lines are only seen for a moment; if larger quantities are contained, the phenomenon lasts for a longer time.

Only in the silicates which are decomposed by hydrochloric acid can the calcium be thus found. In those minerals which

are not attacked by that acid, the following method may be best employed for the detection of calcium. A few milligrammes of the substance under examination, in a state of fine division, are brought upon a flat platinum lid, together with about a gramme of fluoride of ammonium, and the mixture is gently ignited until all the fluoride is volatilized. The slight crust of salt remaining is moistened with a few drops of sulphuric acid, and the excess of acid removed by heat. If about a milligramme of the residual sulphates be scraped together with a knife, and brought into the flame, the characteristic spectra of potassium, sodium, and lithium, supposing these three metals to be present, are first obtained either simultaneously or consecutively. If calcium and strontium be also present, the corresponding spectra generally appear somewhat later, after the potassium, sodium, and lithium have been volatilized. When only traces of strontium and calcium are present, the reaction is not always seen; it becomes, however, immediately apparent on holding the bead for a few moments in the reducing flame and, after moistening it with hydrochloric acid, again bringing it into the flame.

These easy experiments, such as either heating the specimen alone, or after moistening with hydrochloric acid, or after treating the powder with fluoride of ammonium, either alone or in presence of sulphuric or hydrochloric acid, provide the mineralogist and geologist with a series of most simple methods of recognizing the components of the smallest fragment of many substances (such, for instance, as the double silicates containing lime) with a certainty which is attained in an ordinary analysis only by a large expenditure of time and material. The following examples will illustrate this statement.

1. A drop of sea-water heated on the platinum wire shows at first a strong sodium reaction; and after volatilization of the chloride of sodium, a weak calcium spectrum is observed, which on moistening the wire with hydrochloric acid becomes at once very distinct. If a few decigrammes of the residual salts obtained by the evaporation of sea-water be treated in the manner described under lithium with sulphuric acid and alcohol, the potassium and lithium reactions are obtained. The presence of strontium in sea-water can be best detected in the boiler-crust from sea-going steamers. The filtered hydrochloric acid solution of such a crust leaves, on evaporation and subsequent treatment with a small quantity of alcohol, a residue slightly yellow-coloured from basic iron salt, which is deposited after some days, and can then be collected on a small filter and washed with alcohol. The filter, burnt on a fine platinum wire and held in the flame, gives, besides the calcium lines, an intensely bright strontium spectrum.

2. Mineral waters often exhibit the reactions of potassium, sodium, lithium, calcium, and strontium by mere heating. If, for example, a drop of the Dürkheim or Kreuznach waters be brought into the flame, the lines $\text{Na } \alpha$, $\text{Li } \alpha$, $\text{Ca } \alpha$, and $\text{Ca } \beta$ are at once seen. If, instead of using the water itself, a drop of the mother-liquor is taken, these bands appear most vividly. As soon as the chlorides of sodium and lithium have been to a certain extent volatilized, and the chloride of calcium has become more basic, the characteristic lines of the strontium spectrum begin to show themselves, and continue to increase in distinctness, until at last they come out in all their true brightness. In this case, therefore, by the mere observation of a single drop undergoing vaporization, the complete analysis of a mixture containing five constituents is performed in a few seconds.

3. The ash of a cigar moistened with hydrochloric acid, and held in the flame, shows at once the bands $\text{Na } \alpha$, $\text{K } \alpha$, $\text{Li } \alpha$, $\text{Ca } \alpha$, $\text{Ca } \beta$.

4. A piece of hard potash-glass combustion tubing gave, both with and without hydrochloric acid, the lines $\text{Na } \alpha$ and $\text{K } \alpha$; treated with fluoride of ammonium and sulphuric acid, the bands $\text{Ca } \alpha$, $\text{Ca } \beta$, and traces of $\text{Li } \alpha$ were rendered visible.

5. Orthoclase from Baveno gives, either alone or when treated with hydrochloric acid only, the line $\text{Na } \alpha$ with traces of $\text{Li } \alpha$ and $\text{K } \alpha$; with fluoride of ammonium and sulphuric acid, the bright lines $\text{Na } \alpha$ and $\text{K } \alpha$, and a somewhat less distinct $\text{Li } \alpha$, are seen. After volatilization of the bodies thus detected, the bead moistened with hydrochloric acid gives a scarcely distinguishable flash of the lines $\text{Ca } \alpha$ and $\text{Ca } \beta$. The residue on the platinum wire, when moistened with cobalt solution and heated, gives the blue colour so characteristic of alumina. If the well-known reaction of silicic acid be likewise employed, we may conclude from this examination, made in the course of a very few minutes, that the orthoclase from Baveno contains silicic acid, alumina, potash with traces of soda, lime, and lithia; and also that no trace of baryta or strontia is present.

6. Adularia from St. Gothard comported itself in a similar manner, with the exception that the calcium reaction was indistinctly seen, whilst that of lithium was altogether wanting.

7. Labradorite from St. Paul gives the sodium line $\text{Na } \alpha$, but no calcium spectrum. On moistening the fragment with hydrochloric acid, the lines $\text{Ca } \alpha$ and $\text{Ca } \beta$ appear very distinct; with the fluoride of ammonium test, a weak potassium reaction is obtained, and also faint indications of lithium.

8. Labradorite from the Corsican diorite gave similar reactions, except that no lithium was found.

9. Mosanderite from Brevig, and Tscheffkinite from the

Ilmengebirge, showed, when treated alone, the sodium reaction ; on the addition of hydrochloric acid, the lines Ca α and Ca β appeared.

10. Melinophane from Lamoe gave the line Na α when placed alone in the flame ; with hydrochloric acid the lines Ca α , Ca β , and Li α became visible.

11. Scheelite and sphene give, on treatment with hydrochloric acid, a very intense calcium reaction.

12. When small quantities of strontium are present together with calcium, the line Sr δ may be most conveniently employed for the detection of this metal. In this way the presence of small quantities of strontium can be easily detected in very many sedimentary limestones. The lines Na α , Li α , Ka α , especially Li α , are observed as soon as the limestone is brought into the flame. Converted by hydrochloric acid into chlorides, and brought in this form into the flame, these minerals give the same bands ; and not unfrequently the line Sr δ is also distinctly seen. This latter appears, however, only for a short time, and is in general best seen when the calcium spectrum begins to fade.

In this way the lines Na α , Li α , Ka α , Ca α , Ca β , and Sr δ were found in the spectra of the following limestones :—

Limestone from the Silurian at Kugelbad near Prague. Muschelkalk from Rohrbach near Heidelberg. Limestone from the Lias at Malsch in Baden. Chalk from England.

The following limestones gave the lines Na α , Li α , Ka α , Ca α , Ca β , but not the blue strontium band Sr δ :—

Marble from the granite near Auerbach*. Limestone from the Devonian at Gerolstein in the Eifel. Carboniferous limestone from Planitz in Saxony. Dolomite from Nordhausen in the Harz. Jura kalk from Streitberg in Franconia.

From these few experiments it is evident that a more extended series of exact spectrum analyses, respecting the amount of strontium, lithium, sodium, and potassium which the various limestone formations contain, must prove of the greatest geological importance, both as regards the order of their formation and their local distribution, and may possibly lead to the establishment of some unexpected conclusions respecting the nature of the oceans from which these limestones were originally deposited.

Barium.

The barium spectrum is the most complicated of the spectra of the alkalies and alkaline earths. It is at once distinguished

* According to the method already described, a quantity of nitrate of strontium was obtained from 20 grms. of this marble such as to give a complete and vivid strontium spectrum. We have not examined the other limestones in the same way.

from all the others by the green lines Ba α and Ba β (which are by far the most distinct) appearing the first and continuing during the whole of the reaction. Ba γ is not quite so distinct, but is still a well-marked and peculiar line. As the barium spectrum is considerably more extended than those of the other metals, the reaction is not observed to so great a degree of delicacy; still 0.3 grm. of chlorate of barium burnt with milk-sugar gave a distinct band of Ba α which lasted for some time, when the air of the room was well mixed by moving an open umbrella about. Hence we may calculate, in the same manner as was done in the sodium experiment, that about $\frac{1}{1000}$ of a milligramme of barium salt may be detected with the greatest certainty.

The chloride, bromide, iodide, and fluoride of barium, as also the hydrated oxide, the sulphate, and carbonate, show the reaction best. It may be obtained by simply heating any of these salts in the flame.

Silicates containing barium, which are decomposed by hydrochloric acid, also give the reaction if a drop of hydrochloric acid be added to them before they are brought into the flame. Baryta-harmotome, treated in this way, gives the lines Ca α and Ca β , together with the bands Ba α and Ba β .

Compounds of barium with fixed acids, giving no reaction either when alone or after addition of hydrochloric acid, should be fused with carbonate of sodium, as described under strontium, and the carbonate of barium thus obtained examined. If barium and strontium occur in small quantities together with large amounts of calcium, the carbonates obtained by fusion are dissolved in nitric acid, and the dried salt extracted with alcohol. The residue contains only barium and strontium, both of which can almost always be detected. When we wish to test for small traces of strontium or barium, the residual nitrates are converted into chlorides by ignition with sal-ammoniac, and the chloride of strontium is extracted by alcohol. Unless one or more of the bodies to be detected is present in very small quantities, the methods of separation just described are quite unnecessary, as is seen from the following experiment:—

A mixture of the chlorides of potassium, sodium, lithium, calcium, strontium, and barium, containing at the most $\frac{1}{10}$ of a milligramme of each of these salts, was brought into the flame, and the spectra produced were observed. At first the bright yellow sodium line Na α appeared, with a background formed by a nearly continuous pale spectrum. As soon as this line began to fade, the exactly defined bright red line of lithium Li α was seen; and still further removed from the sodium line the faint red potassium line Ka α was noticed, whilst the two barium lines, Ba α , Ba β , with their peculiar form, became visible in the proper

position. As the potassium, sodium, lithium, and barium salts volatilized, their spectra became fainter and fainter, and their peculiar bands one after the other vanished, until, after the lapse of a few minutes, the lines $\text{Ca } \alpha$, $\text{Ca } \beta$, $\text{Sr } \alpha$, $\text{Sr } \beta$, $\text{Sr } \gamma$, and $\text{Sr } \delta$ became gradually visible, and, like a dissolving view, at last attained their characteristic distinctness, colouring, and position, and then, after some time, became pale and disappeared entirely.

The absence of any one or of several of these bodies is at once indicated by the non-appearance of the corresponding bright lines.

Those who become acquainted with the various spectra by repeated observation, do not need to have before them an exact measurement of the single lines in order to be able to detect the presence of the various constituents; the colour, relative position, peculiar form, variety of shade and brightness of the bands are quite characteristic enough to ensure exact results, even in the hands of persons unaccustomed to such work. These special distinctions may be compared with the differences of outward appearance presented by the various precipitates which we employ for detecting substances in the wet way. Just as it holds good as a character of a precipitate that it is gelatinous, pulverulent, flocculent, granular, or crystalline, so the lines of the spectrum exhibit their peculiar aspects, some appearing sharply defined at their edges, others blended off either at one or both sides, either similarly or dissimilarly, or some, again, appearing broader, others narrower; and just as in ordinary analysis we only make use of those precipitates which are produced with the smallest possible quantity of the substance supposed to be present, so in analysis with the spectrum, we employ only those lines which are produced by the smallest possible quantity of substance and require a moderately high temperature. In these respects both analytical methods stand on an equal footing; but analysis with the spectrum possesses a great advantage over all other methods, inasmuch as the characteristic differences of colour of the lines serve as the distinguishing feature of the system. Most of the precipitates which are valuable as reactions are colourless; and the tint of those which are coloured varies very considerably according to the state of division and mechanical arrangement of the particles. The presence of even the smallest quantity of impurity is often sufficient entirely to destroy the characteristic colour of a precipitate; so that no reliance can be placed upon nice distinctions of colour as an ordinary chemical test. In spectrum-analysis, on the contrary, the coloured bands are unaffected by such alteration of physical conditions, or by the presence of other bodies. The positions which the lines occupy in the spectrum give rise to

chemical properties as unalterable as the combining weights themselves, and which can therefore be estimated with an almost astronomical precision. The fact, however, which gives to this method of spectrum-analysis an extraordinary importance is, that the chemical reactions of matter thus reach a degree of delicacy which is almost inconceivable. By an application of this method to geological inquiries concerning the distribution and arrangement of the components of the various formations, the most valuable results may be expected; even the few random experiments already mentioned have led to the unexpected conclusion, that not only potassium and sodium, but also lithium and strontium must be added to the list of bodies occurring, only indeed in small quantities, but most widely spread, throughout the matter composing the solid body of our planet.

The method of spectrum-analysis may also play a no less important part as a means of detecting new elementary substances; for if bodies should exist in nature so sparingly diffused that the analytical methods hitherto applicable have not succeeded in detecting, or separating them, it is very possible that their presence may be revealed by a simple examination of the spectra produced by their flames. We have had opportunity of satisfying ourselves that in reality such unknown elements exist. We believe that, relying upon unmistakeable results of the spectrum-analysis, we are already justified in positively stating that, besides potassium, sodium, and lithium, the group of the alkaline metals contains a fourth member, which gives a spectrum as simple and characteristic as that of lithium—a metal which in our apparatus gives only two lines, namely a faint blue one, almost coincident with the strontium line $\text{Sr } \delta$, and a second blue one lying a little further towards the violet end of the spectrum, and rivalling the lithium line in brightness and distinctness of outline.

The method of spectrum-analysis not only offers, as we flatter ourselves we have shown, a mode of detecting with the greatest simplicity the presence of the smallest traces of certain elements in terrestrial matter, but it also opens out the investigation of an entirely untrodden field, stretching far beyond the limits of the earth, or even of our solar system. For, in order to examine the composition of luminous gas, we require, according to this method, only to see it; and it is evident that the same mode of analysis must be applicable to the atmospheres of the sun and of the brighter fixed stars. A modification must, however, be introduced in respect to the light which these heavenly bodies themselves emit. In a memoir published by one of us*, "On

* Kirchhoff, Poggendorff's *Annalen*, vol. cix. p. 275; and *Phil. Mag.* S. 4. vol. xx. p. 1.

the Relation between the Coefficients of Emission and Absorption of Bodies for Heat and Light," it was proved from theoretical considerations that the spectrum of an incandescent gas becomes *reversed* (that is, that the bright lines become changed into dark ones) when a source of light of sufficient intensity, giving a continuous spectrum, is placed behind the luminous gas. From this we may conclude that the solar spectrum, with its dark lines, is nothing else than the reverse of the spectrum which the sun's atmosphere alone would produce. Hence, in order to effect the chemical analysis of the solar atmosphere, all that we require is to discover those substances which, when brought into the flame, produce bright lines coinciding with the dark ones of the solar spectrum.

In the paper above referred to, the following experimental facts are given in confirmation of the preceding theoretical conclusion.

The bright red line produced in the spectrum of a gas-flame by the presence of a bead of chloride of lithium, is changed into a dark one when direct sunlight is allowed to pass through the flame. When the bead of lithium is replaced by one of chloride of sodium, the dark double line D (coincident with the yellow sodium line) appears with uncommon distinctness.

The dark double line D also appears when the rays of a Drummond's light are passed through the flame of aqueous alcohol into which chloride of sodium is thrown*.

It appeared of interest to obtain still further confirmation of this important theoretical conclusion ; the following experiments answered this purpose :—

We ignited a thick platinum wire in the flame, and then by means of an electric current, heated it to a temperature approaching its melting-point. The wire gave a bright spectrum, in which no trace of either dark or bright lines was seen. A flame of weak aqueous alcohol, in which common salt was dissolved, on being brought between the wire and the slit of the apparatus, gave the dark line D most distinctly.

In the Philosophical Magazine for March 1860, Prof. Stokes calls attention to the fact that in the year 1849 Foucault made an observation very similar to the above. In the examination of the spectrum produced by the electric arc between carbon points, Foucault noticed that bright lines occur where the double line D of the solar spectrum is found, and that this dark line D is produced or made more intense when the rays of the sun, or those from one of the incandescent carbon poles, are passed through the luminous arc. The observation mentioned in the text affords an explanation of this interesting phenomenon observed by Foucault eleven years ago, proving that it is not occasioned by the properties of the electric light, which in many respects is still so enigmatical, but that it arises from a compound of sodium contained in the pole, and converted into incandescent gas by the current.

The dark line D can be produced in the spectrum of a platinum wire heated in a flame, by holding between flame and spectrum a test-tube containing some sodium amalgam which is heated to boiling. This experiment is important, because it shows that sodium vapour at a temperature much below that at which it becomes luminous, exerts its absorptive power at exactly the same point of the spectrum as it does at the highest temperatures which we can produce, or at the temperatures existing in the solar atmosphere.

We have succeeded in reversing the bright lines in the spectra of Ka, Sr, Ca, Ba, by employing sunlight and mixtures of the chlorates of these metals with milk-sugar. A small iron trough was fixed in front of the slit of our apparatus, in which the mixture was placed; the direct sunlight was then allowed to fall along the whole length of the trough, and the mixture ignited with a heated wire. The telescope C, with the wires cutting each other at an acute angle, was placed so that the point of intersection of the wires coincided with the bright line of the flame-spectrum which was to be examined. The observer concentrated his attention upon this point, to judge whether, at the moment of burning the mixture, a dark line showed itself passing through the point of intersection of the cross wires. In this way it was easy, when the right proportions for the mixtures were found, to show that the lines Ba α , Ba β , as well as the line Ka β , were reversed. The last of these lines coincides with one of the most distinct dark lines in the solar spectrum, though not marked by Fraunhofer, which, however, appears much more plainly than it is generally seen at the moment the potash salt burns. In order to prove that the strontium lines can be reversed, the chlorate of strontium must be most carefully dried, as the slightest trace of moisture produces a positive strontium spectrum, owing to the small particles of salt being thrown about in the flame, and thus diminishing the power of the solar rays.

We have, in the present memoir, contented ourselves with such an examination of the spectra of the metals of the alkalis and alkaline earths as was necessary for the analysis of terrestrial matter. We reserve for a future communication the further applications of the spectrum-analytical method to terrestrial matter, and to the analysis of the atmospheres of the stars.

X. *On the Bases produced by the Destructive Distillation of Peat.*
By ARTHUR H. CHURCH and EDWARD OWEN*.

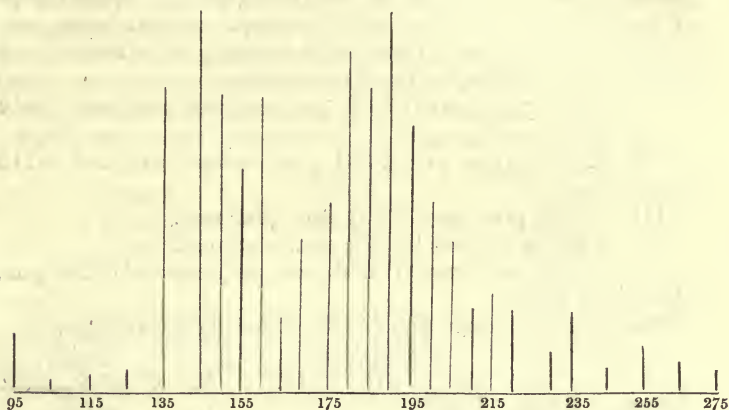
THE crude material which furnished the bases described in the present communication was obtained by the destructive distillation of Irish peat. The operation was performed in a blast-furnace, according to the patent of Mr. Rees Reece, the carbonaceous residues of former operations constituting the fuel employed. The distillation was conducted at the lowest possible temperature, avoiding thereby any undue carbonization of the turf prior to the formation and liberation of its volatile products. Close to the nozzle or tuyère of the blast, the heat, it is true, is intense; but the carbonic acid there produced passing up the furnace through a zone of carbon, is reduced in temperature and converted into carbonic oxide, while this gas ascending higher carries off the tarry products of the turf at a temperature of some 200° C. only. The bases upon which we have been experimenting were contained in the distillate of a tar thus produced in the destructive distillation of peat in an atmosphere of carbonic oxide and marsh-gas at the lowest possible temperature. According to the researches of Kane and Sullivan, the tarry products from the various kinds of peat differ but very inconsiderably in their ultimate composition.

The process adopted in order to extract the bases from the peat distillate was essentially the same as that employed by Hofmann, and described by him in his papers on the organic bases contained in coal-gas naphtha. The oil, about 400 gallons, being the product from the distillation of 100 tons of peat, was shaken with hydrochloric acid, the solution drawn off, and this operation repeated until no more bases were thus extracted. The pyrrol and other impurities in the acid solution were then removed by long-continued ebullition, and evaporation to a small bulk. The liquid was then filtered, introduced into a still, and supersaturated with lime. The bases that came over on distillation were again combined with hydrochloric acid, and separated anew by distillation with excess of potash. They were then thoroughly dried with caustic potash, and submitted to repeated fractional distillations. The ammonia (the quantity of which yielded by peat is comparatively trifling) and any very volatile bases present had been previously eliminated in the original distillation of the peat, and occurred in the watery fluid which accompanied the oils.

The following sketch, which represents very roughly the amount of base which came over in the 8th rectification at intervals of 5° C., or in some cases of 10°, may serve to give some

* Communicated by the Authors.

idea of the proportionate quantities of the various bases present, for it was found that these proportions were not very materially altered even after the tenth rectification.



Between 500 and 600 distillations were performed during the course of the present investigation, and the expenditure of material, time, and labour has been very large. We could have wished to offer an exhaustive account of the complex basic fluid which we have worked upon; the entire absence of any previous knowledge of the bases produced by the destructive distillation of peat must be our excuse for the imperfections of the present memoir.

A few tentative experiments were first made as to the nature and composition of the basic oils by means of their platinum salts. But our attention was soon mainly directed to the most volatile fraction collected between 95° and 97° , and the boiling-point of which was almost constant at 97° . This liquid was in reality separated by an interval of 23° from the next higher fraction, while any more volatile bases that might originally have been present in the distilled oil had been lost in the various operations to which it had been subjected. To this body, which subsequent examination proved to be a pure base, we have assigned the name *cespitine*. We proceed to detail our analyses and experiments, and to give some account of the properties of the new base and its derivatives.

Cespitine.

The fraction 95° to 100° of the 8th rectification was dried thoroughly with caustic potash and redistilled,—the portion coming over between 95° and 97° , which constituted $\frac{9}{10}$ ths of the whole, being employed for experiment. A considerable

quantity was dissolved in water, hydrochloric acid added, and then bichloride of platinum; sufficient water to redissolve the orange-yellow crystalline precipitate first formed was then poured in and the mixture set aside to crystallize. Several crops of magnificent orange-red shuttle-shaped crystals were successively obtained, and these, after washing with alcohol and ether, gave the following results on analysis:—

I. 0.325 grm. gave 0.2432 grm. carbonic acid and 0.1414 grm. water.

II. 0.2705 grm. gave 0.201 grm. carbonic acid and 0.119 grm. water.

III. 0.3008 grm. gave 0.101 grm. platinum.

IV. 0.409 grm. gave 0.1378 grm. platinum.

V. 0.1925 grm. gave 0.0648 grm. platinum and 0.283 grm. chloride of silver.

These numbers correspond to the following per-centages:—

	Experiment.					Theory,
	I.	II.	III.	IV.	V.	$C^{10}H^{14}NCl, PtCl_2$.
Carbon	20.41	20.26	20.43
Hydrogen	4.83	4.89	4.77
Nitrogen	4.77
Chlorine	36.36	36.29
Platinum	33.57	33.69	33.66	33.74*
						100.00

The platinum salt analysed had therefore the composition of that of amylamine; our reasons for thinking it isomeric but not identical with that substance, will be found below. We place but little reliance upon any distinctions based upon the physical characteristics only of the two salts, such characteristics being singularly variable in the case of many platinum salts. A few brilliant yellow spangles finally separated from the mother-liquor of the cespitine platinum compound, and gave the following result when burnt:—

0.1786 grm. gave 0.0618 grm. platinum.

This corresponds to 34.60 per cent. of platinum, and agrees with the per-centage of platinum in the pyridine double salt—34.68.

Our earliest determinations of platinum in the platinum salt of the fraction 95° — 100° , gave the following numbers:—

I. 0.3121 grm. yielded 0.104 grm. platinum = 33.29 p. c.

II. 0.4235 grm. yielded 0.1418 grm. platinum = 33.29 p. c.

On recrystallizing the remaining salt and igniting, the following results were obtained:—

III. 0.3115 grm. gave 0.1055 grm. platinum = 33.86 p. c.

IV. 0.3173 grm. gave 0.1085 grm. platinum = 34.19 p. c.

* 99 is the equivalent of platinum adopted in this memoir.

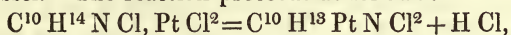
These numbers agree pretty closely with those required by the theory, $C^{10}H^{14}NCl, PtCl^2$, while the excess of platinum obtained in analyses III. and IV., made with a salt recrystallized by the aid of heat, is probably to be explained by the result of an experiment in which we have proved that the cespitine platinum salt yields a platinized derivative when boiled with water. Before proceeding to describe this new substance, we give the results furnished by an analysis of the pure base cespitine.

0.237 grm. gave 0.598 grm. carbonic acid and 0.32 grm. water.

	Experiment.	Theory, $C^{10}H^{13}N$.
Carbon	68.81	68.96
Hydrogen	15.00	14.95
Nitrogen	..	16.09
		<hr/> 100.00

The platinum salt of cespitine, which is but slightly soluble in cold water, dissolves readily in boiling water; and the solution, if kept in steady ebullition for half an hour, deposits brilliant pale yellow scales, the supernatant liquid becoming colourless.

During this transformation hydrochloric acid is evolved in considerable quantity, and may be readily detected at the mouth of the vessel. The reaction proceeds as follows:—



and is perfectly analogous to the change produced in the platinum salts of pyridine and picoline by ebullition with water, although it is effected with much greater ease, and its production is not preceded by that of a double compound containing one equivalent of the original united with one of the changed salt.

The composition of the new crystals was ascertained by the analysis given below:—

I. 0.3715 grm. gave 0.316 grm. carbonic acid and 0.2085 grm. water.

II. 0.42 grm. gave 0.1605 grm. platinum.

III. 0.2595 grm. gave 0.099 grm platinum.

IV. 0.2018 grm. gave 0.077 grm. platinum.

	Experiment.				Theory,
	I.	II.	III.	IV.	$(C^{10}H^{13})'''PtNCl^2$.
Carbon	23.20	23.34
Hydrogen	6.23	5.06
Platinum	..	38.21	38.11	38.16	38.52
Nitrogen	5.45
Chlorine	27.63
					<hr/> 100.00

An attempt to prepare the bichloride of plato-cespityl-ammo-

nium, for so we provisionally name the salt just described, by the direct union of cespitine and bichloride of platinum, led to no definite result. The bichloride of plato-cespityl-ammonium is not altered by continued ebullition, but may be crystallized from boiling water.

The metamorphosis of the platinum salt of cespitine pointed to an essential difference between that base and amylamine; and this difference is the more remarkable, since the true amylamine has been detected by Greville Williams and by Anderson among the products of the destructive distillation of several organic substances. An experiment was made, however, in order to establish this distinction more conclusively. One volume of pure cespitine was digested in a sealed tube at 120°C . for several days with 8 or 10 volumes of iodide of ethyle. On opening the tube no escape of hydriodic acid gas occurred, but on shaking up its contents with water, and evaporating the aqueous solution, a syrup was obtained which did not show the least tendency to crystallize even *in vacuo* over sulphuric acid; the iodide of ethyl-amyl-ammonium is easily crystallizable. On the addition of oxide of silver to this syrup after it had been diluted with water, an alkaline and caustic liquid was obtained, but no volatile base separated; had the cespitine been in reality amylamine, ethyl-amylamine should have been formed, together with other bases containing more ethyle. But no such substitution had taken place; the cespitine, being a nitrile base containing no hydrogen thus replaceable, had merely combined with 1 equiv. of iodide of ethyle to form a salt. The alkaline solution of the ethyle derivative was now neutralized with hydrochloric acid, and precipitated with bichloride of platinum; a buff-coloured curdy precipitate was formed, which, when washed and then recrystallized from warm water, yielded very voluminous micaceous plates of a straw colour. These crystals were almost insoluble in cold water. The following determinations proved them to be the platinum salt of ethyl-cespityl-ammonium:—

- I. 0.4295 grm. gave 0.132 grm. platinum.
- II. 0.338 grm. gave 0.1037 grm. platinum.
- III. 0.5078 grm. gave 0.1555 grm. platinum.

	Experiment.			Theory,
	I.	II.	III.	($\text{C}^{10}\text{H}^{13}$) ^{'''} , $\text{C}^4\text{H}^2, \text{N Cl}, \text{Pt Cl}^2$.
Platinum	30.73	30.69	30.62	30.79

When cespitine is acted on by iodide of amyle, a yellow uncrystallizable gummy mass is obtained, the iodide of amyl-cespityl-ammonium ($\text{C}^{10}\text{H}^{13}$)^{'''}, $\text{C}^{10}\text{H}^{11}, \text{N Cl}, \text{Pt Cl}^2$, very different from the isomeric body, the crystalline iodide of diamyl-ammonium, produced by the action of iodide of amyle on

amylamine. And when this iodide was acted upon by oxide of silver, and the hydrate thus produced combined with hydrochloric acid, an excessively soluble chloride was obtained, which did not exhibit a trace of crystallization when evaporated to dryness, and had in fact no character in common with that of the highly crystalline and difficultly soluble chloride of diamylammonium.

Cespitine is isomeric not only with amylamine, but also with diethylmethylaniline and dimethylpropylaniline; and since it has been found to contain no replaceable hydrogen, it seemed probable that it might be one of the nitryles just mentioned. But cespitine is entirely unacted upon by nitrous acid, not a trace of any alcohol being produced, and the original base being recoverable by distillation with potash. In our ignorance of the true constitution of cespitine we have supposed it to contain the triatomic radical $(C^{10}H^{13})'''$, although the unusually large proportion of hydrogen casts a doubt upon such a view. All our experiments with cespitine lead us to believe that it bears the same relation to the true amylamine as picoline does to aniline.

Cespitine is a colourless oil, less fluid than water, and miscible in all proportions with that liquid. It is nearly insoluble in a saturated solution of caustic potash. It boils at about 95° , the boiling-point of amylamine its isomer. It is lighter than water; its odour is powerful, and although slightly unpleasant, much less so than amylamine. It precipitates many metallic solutions; with sulphate of copper it gives a green precipitate, soluble in excess of the base with a *pale green* colour. With chloride of mercury a beautiful pearly substance is deposited in iridescent scales. The platinum double salt has been already described: the gold salt is a pale yellow crystalline powder. The cadmium double salt is very soluble, but it may be obtained in long colourless prisms by mixing strong solutions of hydrochlorate of cespitine and chloride of cadmium, and then evaporating the liquid over sulphuric acid.

Pyridine.

It will be seen from our further examination of the bases from peat, that they are identical in composition with pyridine and its homologues. And since we have not thought it necessary to describe them minutely, we may here state that their properties, physical and chemical, correspond closely with those of pyridine, picoline, &c., as recorded by Anderson.

The boiling-point assigned to pyridine is $116^{\circ}.5$. The few drops of base which distilled near this point, coming over in the 8th rectification between 110° and 120° , were converted into the hydrochlorate, and precipitated with an insufficient quantity of

bichloride of platinum : this first crop of platinum salt gave the following result when burnt :—

0·495 grm. gave 0·1708 grm. platinum.

Reducing this number to a per-centage, we have

	Experiment.	Theory—Pyridine.
Platinum	34·50	34·68.

The filtrate from the pyridine salt yielded another crop of crystals on the addition of more bichloride of platinum ; the amount was, however, insufficient for an analysis.

The pyridine platinum salt yielded pale yellow flocks of the bichloride of plato-pyridyl-ammonium after having been boiled with water for forty-eight hours.

Picoline.

Between 130° and 140°, rather more than 70 grammes were obtained in the 9th rectification ; picoline is said to boil at 135°. This fraction proved to be pure picoline. Converted into a platinum salt,—

I. 0·5824 grm. gave 0·1922 grm. platinum = 33·01 p. c.

Between 140° and 145° 100 grammes distilled over, and, like the preceding fraction, were pure picoline. Three successive crops of the platinum salt fractionally precipitated, gave the following numbers :—

II. 0·3755 grm. gave 0·1238 grm. platinum = 32·97 p. c.

III. 0·441 grm. gave 0·1455 grm. platinum = 32·98 p. c.

IV. 0·278 grm. gave 0·0916 grm. platinum = 32·95 p. c.

The platinum salts of the next fraction gave slightly different numbers, indicating a mixture of picoline and lutidine. The oil had been collected between 145° and 150°, and amounted to not less than 60 grammes.

V. 0·292 grm. gave 0·0958 grm. platinum = 32·81 p. c.

VI. 0·3056 grm. gave 0·1008 grm. platinum = 32·98 p. c.

VII. 0·5002 grm. gave 0·1615 grm. platinum = 32·29 p. c.

VIII. 0·51 grm. gave 0·1632 grm. platinum = 32·00 p. c.

IX. 0·3372 grm. gave 0·109 grm. platinum = 32·03 p. c.

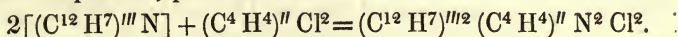
X. 0·4816 grm. gave 0·1516 grm. platinum = 31·47 p. c.

The following is a comparison of the ten preceding experimental per-centages of platinum, with the theoretical per-centages required by picoline and lutidine :—

	Experiment.										Theory.	
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	Picoline.	Lutidine.
	33·01	32·97	32·98	32·95	32·81	32·98	32·29	32·00	32·03	31·47	33·06	31·58

The quantity of pure picoline at our disposal has induced us to experiment further upon it, in order to obtain some insight into its true constitution. By the action of the bichloride or

the bibromide of ethylene upon picoline, we have obtained salts of an ammonium which is remarkable for the beauty of many of its compounds. The reaction, which takes place readily when the materials enclosed in a sealed tube are heated to the boiling-point of picoline, proceeds thus:—



Two equivalents of picoline have thus been united into one molecule by means of the diatomic group ethylene: at the same time hydrochlorate of picoline and other compounds appear to be formed, but only in small quantity. The details concerning several new derivatives of the pyridine bases are reserved for another communication.

Lutidine.

The fraction 150° to 155° was not examined. The fraction 155° to 160° amounted to 60 grammes, and consisted of pure lutidine, the boiling-point of which is 154°. The following determinations of metal in the platinum salt agree well with the theory:—

I. 0.0728 grm. gave 0.0228 grm. platinum = 31.33 p. c.

II. 0.605 grm. gave 0.19 grm. platinum = 31.40 p. c.

A portion of the base which distilled over in the 8th rectification between 160° and 165°, gave a platinum salt which on ignition furnished the following numbers: it had been recrystallized from alcohol, and was a very beautiful salt:—

III. .5062 grm. gave 0.1585 grm. platinum = 31.31 p. c.

IV. .506 grm. gave 0.1585 grm. platinum = 31.32 p. c.

These platinum salts, which were of a deep orange colour and highly crystalline, occurring in very regularly flattened square tables, corresponded almost exactly with the lutidine platinum salt as described by Anderson, but seemed to be rather less soluble in cold water.

Translated into per-centages, the four preceding analyses give the following results:—

	Experiment.				Theory.
	I.	II.	III.	IV.	Lutidine.
Platinum	31.33	31.4	31.31	31.32	31.58

The fraction 165°—170° gave indication of the presence of the next higher homologue of lutidine when the platinum salts fractionally precipitated were ignited.

V. 0.334 grm. gave 0.1052 grm. platinum = 31.52 p. c.

VI. 0.4345 grm. gave 0.1346 grm. platinum = 30.97 p. c.

VII. 0.291 grm. gave 0.0884 grm. platinum = 30.37 p. c.

The per-centage of platinum in the last of these salts is almost

exactly the same as that in the salt of collidine which contains 30·23 per cent. platinum.

Collidine.

The fraction 170°—175° was a mixture of lutidine and collidine, while the liquids that distilled between 175°—180°, 180°—185°, 185°—190° were nearly pure collidine, which is stated to boil at 180°. The three annexed determinations were made with the platinum salts obtained from these three fractions.

- I. 0·4162 grm. gave 0·1256 grm. platinum = 30·17 p. c.
- II. 0·383 grm. gave 0·1153 grm. platinum = 30·10 p. c.
- III. 0·354 grm. gave 0·107 grm. platinum = 30·22 p. c.

Experiment.			Theory.
I.	II.	III.	Collidine.
30·17	30·10	30·22	30·23

The largest quantity of collidine distilled over between 180° and 186°; altogether about 200 grammes were collected. The entire absence of aniline from the peat bases was proved by several careful qualitative experiments; while the platinum determination made with the salt of the 180°—185° fraction (III. above) gave a per-centage differing from that of the aniline salt by as much as 2·84 per cent. In this fraction, aniline, the boiling-point of which is 182°, should have been found had it been present.

The examination of the fractions above 190° and up to 210° has not led to any very definite result. Not only are the oils above this point difficult of combustion, but they are not capable of furnishing crystalline salts. Concentrated hydrochloric solutions of the various fractions which came over in the 10th rectification between 190° and 215°, gave no precipitate with a strong solution of bichloride of platinum, even after the addition of alcohol and ether; nor could any double compound be prepared, either with the chloride of uranyle or of cadmium. The compounds produced with sulphuric and oxalic acid are colourless gummy masses. But all the fractions give with chloride of gold a bright yellow precipitate, which quickly collects at the bottom of the vessel as a brownish-yellow exceedingly heavy oil. This oil is slightly soluble in water, and perfectly soluble in a mixture of alcohol and ether. Two specimens of this gold salt were prepared and the gold determined. The first determination was made with a salt obtained from the fraction boiling between 202° and 205°, which after washing with water was dried *in vacuo* over sulphuric acid until constant, for otherwise gold was reduced at a very slight elevation of temperature. The second determination was made with a salt similarly prepared from the fraction 205°—208°, but

it had been dissolved in a mixture of alcohol and ether. These fractions were those of the 10th rectification.

I. 0.2404 grm. gave 0.0928 grm. gold.

II. 0.5296 grm. gave 0.2038 grm. gold.

Translated into per-centages, these numbers correspond with the proportion of gold required by a gold salt of parvoline ($C^{18}H^{13}N$) containing four equivalents of water:—

Experiment.		Theory.
I.	II.	$C^{18}H^{14}NCl, AuCl^3 + 4Aq.$
38.60	38.46	38.55

The unpromising character of the salts of these fractions has, however, deterred us from making further experiments with them.

Between 210° and 230° the quantity of distillate was but small, and we have as yet gained no insight into its constitution. From 230° to 280° or 290° several grammes of a thick brownish oil came over; these fractions yielded crystalline platinum salts; it is very probable that they contain chinoline and its higher homologues, lepidine and cryptidine, bases discovered by Greville Williams among the products obtained by the destructive distillation of cinchonine.

The following formulæ are those of the new compounds described in the present memoir.—

Cespitine	$(C^{10}H^{13})'''N.$
Chloride of cespityl-ammonium	$(C^{10}H^{13})'''HNCl.$
Platinum salt of cespityl-ammonium	$(C^{10}H^{13})'''HNCl, PtCl^2.$
Bichloride of plato-cespityl-ammonium.	$(C^{10}H^{13})'''PtNCl^2.$
Chloride of amyl-cespityl-ammonium	$(C^{10}H^{13})'''C^{10}H^{11}NCl.$
Chloride of ethyl-cespityl-ammonium	$(C^{10}H^{13})'''C^4H^5NCl.$
Hydrate of ethyl-cespityl-ammonium	$(C^{10}H^{13})'''C^4H^5NO, HO.$
Platinum salt of ethyl-cespityl-ammonium	$(C^{10}H^{13})'''C^4H^5NCl, PtCl^2.$
Bichloride of ethylene-dipicolyl-diammonium	$(C^{12}H^7)''^2 \left. \begin{array}{l} \\ \end{array} \right\} N^2Cl^2.$
	$(C^4H^4)'' \left. \begin{array}{l} \\ \end{array} \right\}$

XI. On a New Species of Figures of Equilibrium for Revolving Fluids, the particles of which attract one another according to Newton's Theory. By G. R. DAHLANDER*.

SUPPOSE a hollow ellipsoid of rotation to revolve round its figure-axis with a constant velocity. Let it be bounded within by another ellipsoid of rotation, concentric with the first one and having the same axis. Around the inner walls there is a fluid, which does not, however, fill up the whole of the hollow ellipsoid, but forms within a cavity round the centre. By attraction and centrifugal force the fluid can, under certain circumstances, form a figure of equilibrium, which we will now consider.

Let the axis of rotation be the z axis of a rectangular coordinate

* Communicated by the Author.

system, whose origin is in the centre of the ellipsoid. It has now to be shown that an ellipsoid of rotation can form the limiting surface for the cavity within the fluid, supposing the pressure is not directed against the cavity; or if such is the case, that it is counterbalanced by the pressure of a body of gas enclosed in the cavity, whose density is, however, so small that the attraction exerted by the gas on the fluid may be neglected. Let X, Y, Z denote the three components parallel with the co-ordinate axes of the attraction which the ellipsoid and the fluid exercise on a particle of the fluid having the coordinates x, y, z . The attraction of the mass of the hollow ellipsoid on the point x, y, z is equal to that of a solid ellipsoid, diminished by that which another solid ellipsoid equal to the hollow part would exert on the point. Let the components of attraction of the former ellipsoid be

$$-Mx, \quad -My, \quad -Nz,$$

and those of the latter

$$-M'x, \quad -M'y, \quad -N'z.$$

The attraction of the fluid, which is supposed to be bounded by the two spheroids, is equal to that which an ellipsoid of massive fluid would exercise, diminished by that of an ellipsoid with the same density corresponding to the inner cavity. Let the components of the former attraction be

$$-M''x, \quad -M''y, \quad -N''z,$$

and those of the latter

$$-M'''x, \quad -M'''y, \quad -N'''z.$$

Then we get

$$\left. \begin{aligned} X &= (-M + M' - M'' + M''')x, \\ Y &= (-M + M' - M'' + M''')y, \\ Z &= (-N + N' - N'' + N''')z. \end{aligned} \right\} \quad \dots (1)$$

The differential equation of the *surfaces de niveau* will therefore be

$$(-M + M' - M'' + M''')(xdx + ydy) + (-N + N' - N'' + N''')zdz + w^2(xdx + ydy) = 0, \quad \dots (2)$$

where w is the constant angular velocity.

M, M', M'', N, N', N'' are independent of x, y, z ; but M''' and N''' are generally dependent upon them. When the innermost bounding surface is alone considered, even M''' and N''' become independent of x, y, z . If this surface be a spheroid, it will be possible to express its equation in the form

$$\frac{x^2 + y^2}{a^2} + \frac{z^2}{a^2(1 + \lambda'^2)} = 1. \quad \dots (3)$$

Differentiating this equation, we get

$$x dx + y dy + \frac{z dz}{1 + \lambda'^2} = 0. \quad (4)$$

In order that the equations (2) and (4) may both exist at the same time, it is necessary that

$$\frac{-M + M' - M'' + M''' + w^2}{-N + N' - N'' + N'''} = 1 + \lambda'^2. \quad (5)$$

The equation (5) shows the relation that must exist between the several quantities in order that the inner bounding surface may form a spheroid, whereby it is supposed, as before mentioned, that the pressure on the surface is either counter-balanced by the pressure of a body of gas, or is = 0 in consequence of attraction and centrifugal force. The general examination of the circumstances which occur in this case is particularly complicated, as the equation (2) cannot be integrated; we shall therefore confine ourselves to the single case in which an integration can be effected, when the inner bounding surface of the fluid becomes a sphere, and both the outer spheroids are oblong.

Let ρ denote the density of the solid part of the ellipsoid, and ρ' that of the fluid; f the attraction between two unit masses at the unit of distance; and let λ and λ' have the same signification for the two outer spheroids as λ'' has in the equation (3). Further, let r be the radius of the spherical cavity. Then

$$\left. \begin{aligned} M &= 2\pi\rho f \frac{\sqrt{1+\lambda^2}}{\lambda^3} \left[\lambda\sqrt{1+\lambda^2} - l.(\lambda + \sqrt{1+\lambda^2}) \right], \\ N &= 4\pi\rho f \frac{\sqrt{1+\lambda^2}}{\lambda^3} \left[l.(\lambda + \sqrt{1+\lambda^2}) - \frac{\lambda}{\sqrt{1+\lambda^2}} \right], \\ M' &= 2\pi\rho f \frac{\sqrt{1+\lambda'^2}}{\lambda'^3} \left[\lambda'\sqrt{1+\lambda'^2} - l.(\lambda' + \sqrt{1+\lambda'^2}) \right], \\ N' &= 4\pi\rho f \frac{\sqrt{1+\lambda'^2}}{\lambda'^3} \left[l.(\lambda' + \sqrt{1+\lambda'^2}) - \frac{\lambda'}{\sqrt{1+\lambda'^2}} \right], \\ M'' &= 2\pi\rho' f \frac{\sqrt{1+\lambda'^2}}{\lambda'^3} \left[\lambda'\sqrt{1+\lambda'^2} - l.(\lambda' + \sqrt{1+\lambda'^2}) \right], \\ N'' &= 4\pi\rho' f \frac{\sqrt{1+\lambda'^2}}{\lambda'^3} \left[l.(\lambda' + \sqrt{1+\lambda'^2}) - \frac{\lambda'}{\sqrt{1+\lambda'^2}} \right], \\ M''' &= N''' = \frac{4}{3}\pi\rho' f \frac{r^3}{\sqrt{(x^2+y^2+z^2)^3}}. \end{aligned} \right\} \quad (6)$$

If we now take $\frac{w^2}{2\pi\rho f} = E$, $\frac{\rho}{\rho'} = m$, we find the differential equation for the *surfaces de niveau* to be

$$\begin{aligned} & \left\{ -m \frac{\sqrt{1+\lambda^2}}{\lambda^3} \left[\lambda \sqrt{1+\lambda^2} - l \cdot (\lambda + \sqrt{1+\lambda^2}) \right] + (m-1) \frac{\sqrt{1+\lambda'^2}}{\lambda'^3} \left[\lambda' \sqrt{1+\lambda'^2} \right. \right. \\ & \quad \left. \left. - l \cdot (\lambda' + \sqrt{1+\lambda'^2}) \right] + \frac{2}{3} \frac{r^3}{(\sqrt{x^2+y^2+z^2})^3} + E \right\} (x dx + y dy) \\ & + \left\{ -2m \frac{\sqrt{1+\lambda^2}}{\lambda^3} \left[l \cdot (\lambda + \sqrt{1+\lambda^2}) - \frac{\lambda}{\sqrt{1+\lambda^2}} \right] \right. \\ & + 2(m-1) \frac{\sqrt{1+\lambda'^2}}{\lambda'^3} \left[l \cdot (\lambda' + \sqrt{1+\lambda'^2}) - \frac{\lambda'}{\sqrt{1+\lambda'^2}} \right] \\ & \left. + \frac{2}{3} \frac{r^3}{(\sqrt{x^2+y^2+z^2})^3} \right\} z dz = 0. \quad (7) \end{aligned}$$

But for the *surface de niveau* which forms the inner bounding surface $\sqrt{x^2+y^2+z^2} = r$, and we have therefore for it

$$\begin{aligned} & \left\{ -m \frac{\sqrt{1+\lambda^2}}{\lambda^3} \left[\lambda \sqrt{1+\lambda^2} - l \cdot (\lambda + \sqrt{1+\lambda^2}) \right] + (m-1) \frac{\sqrt{1+\lambda'^2}}{\lambda'^3} \left[\lambda' \sqrt{1+\lambda'^2} \right. \right. \\ & \quad \left. \left. - l \cdot (\lambda' + \sqrt{1+\lambda'^2}) \right] + \frac{2}{3} + E \right\} (x dx + y dy) + \left\{ -2m \frac{\sqrt{1+\lambda^2}}{\lambda^3} \left[l \cdot (\lambda + \sqrt{1+\lambda^2}) \right. \right. \\ & \quad \left. \left. - \frac{\lambda}{\sqrt{1+\lambda^2}} \right] + 2(m-1) \frac{\sqrt{1+\lambda'^2}}{\lambda'^3} \left[l \cdot (\lambda' + \sqrt{1+\lambda'^2}) \right. \right. \\ & \quad \left. \left. - \frac{\lambda'}{\sqrt{1+\lambda'^2}} \right] + \frac{2}{3} \right\} z dz = 0. \quad (8) \end{aligned}$$

But for the corresponding values of x, y, z we have the relation

$$x dx + y dy + z dz = 0. \quad (9)$$

By comparing the equations (8) and (9), therefore, we get

$$\begin{aligned} E = & \frac{m(3+\lambda^2)}{\lambda^2} - \frac{(m-1)(3+\lambda'^2)}{\lambda'^2} - \frac{3m\sqrt{1+\lambda^2}}{\lambda^2} l \cdot (\lambda + \sqrt{1+\lambda^2}) \\ & + \frac{3(m-1)\sqrt{1+\lambda'^2}}{\lambda'^3} l \cdot (\lambda' + \sqrt{1+\lambda'^2}). \quad (10) \end{aligned}$$

This relation must exist between the several quantities in order that the inner bounding surface may be a sphere. If we now take

$$\begin{aligned} & -\frac{2m\sqrt{1+\lambda^2}}{\lambda^3} \left[l \cdot (\lambda + \sqrt{1+\lambda^2}) - \frac{\lambda}{\sqrt{1+\lambda^2}} \right] \\ & + 2(m-1) \frac{\sqrt{1+\lambda'^2}}{\lambda'^3} \left[l \cdot (\lambda' + \sqrt{1+\lambda'^2}) - \frac{\lambda'}{\sqrt{1+\lambda'^2}} \right] = P, \quad (11) \end{aligned}$$

the differential equation for the *surfaces de niveau* in general, according to equation (7), will be

$$\left(P + \frac{2}{3} \frac{r^3}{\sqrt{(x^2 + y^2 + z^2)^3}}\right)(xdx + ydy + zdz) = 0. \quad (12)$$

This shows that the *surfaces de niveau* become all spherical. If p denote the pressure on a point of the fluid with the coordinates x, y, z , we have

$$\frac{dp}{2\pi\rho^{1/2}f} = \left(P + \frac{2}{3} \frac{r^3}{(\sqrt{x^2 + y^2 + z^2})^3}\right)(xdx + ydy + zdz). \quad (13)$$

If the radius of the spherical surfaces of niveau is supposed to be n , whereof $xdx + ydy + zdz = ndn$, this equation will be

$$\frac{dp}{2\pi\rho^{1/2}f} = Pndn + \frac{2}{3} \frac{r^3}{n^2} dn,$$

which, when integrated, gives

$$\frac{p}{2\pi\rho^{1/2}f} = \frac{Pn^2}{2} - \frac{2}{3} \frac{r^3}{n} + C.$$

If we now suppose that the equilibrium is only sustained by the attraction and the centrifugal force acting on the particles, the pressure on the inner surface of the fluid must be $=0$. If we determine the arbitrary constant by this condition, we get, lastly,

$$p = 2\pi\rho^{1/2}f \left(\frac{P(n^2 - r^2)}{2} + \frac{2}{3} r^2 \left(1 - \frac{r}{n} \right) \right). \quad (14)$$

In order to examine the circumstances under which equilibrium can be sustained by attraction and centrifugal force, we shall suppose an infinitely small canal in the fluid with a constant sectional area going from the exterior surface of the fluid to its inner surface. We may suppose this canal to be separated from the other fluid by an infinitely thin tube; and if now the acting forces tend to conduct the fluid in the canal towards the cavity, no equilibrium is possible under the supposed circumstances. But if, on the contrary, the resultant of these forces, taken with regard to all the particles of fluid that are in the canal, tends to conduct the fluid towards the solid ellipsoid, equilibrium may exist if at the same time the forces acting on the particles upon the inner surface are also directed towards the solid ellipsoid. As now the *surfaces de niveau* are spherical, it is sufficient to examine a canal going in the direction of the axis of revolution. If the forces acting on the particles in this canal are directed outward, this must be still more the case with the other canals.

If now c denote half the major axis of the inner spheroid,

these conditions are expressed by

$$\int_r^c \left(P + \frac{2}{3} \frac{r^3}{z^2} \right) z dz \geq 0, \quad . \quad . \quad . \quad (15)$$

which, after integration gives

$$\frac{P}{2} \left(1 + \frac{r}{c} \right) + \frac{2}{3} \frac{r^2}{c^2} \geq 0. \quad . \quad . \quad . \quad (16)$$

If this condition is fulfilled, it is clear that the force which acts upon a particle on the inner surface of the fluid, must also be directed outward from the cavity.

Let us now, lastly, consider the special case when $m=1$, *i. e.* when the density of the hollow spheroid is equal to that of the fluid. The equation (11) gives then

$$P = -2 \frac{\sqrt{1+\lambda^2}}{\lambda^3} \left[l. (\lambda + \sqrt{1+\lambda^2}) - \frac{\lambda}{\sqrt{1+\lambda^2}} \right]. \quad . \quad (17)$$

If the smaller semiaxis of the inner spheroid be equal to the radius of the surface of the sphere, we get $\frac{r}{c} = \frac{1}{\sqrt{1+\lambda'^2}}$, and the condition (16) is then expressed by

$$\begin{aligned} & -\frac{\sqrt{1+\lambda^2}}{\lambda^3} \left[l. (\lambda + \sqrt{1+\lambda^2}) - \frac{\lambda}{\sqrt{1+\lambda^2}} \right] \left(1 + \frac{1}{\sqrt{1+\lambda'^2}} \right) \\ & + \frac{2}{3} \frac{1}{1+\lambda'^2} \geq 0. \quad . \quad . \quad . \quad (18) \end{aligned}$$

The equation of condition (10) will in that case be

$$E = \frac{\lambda^2 + 3}{\lambda^2} - \frac{3\sqrt{1+\lambda^2}}{\lambda^3} l. (\lambda + \sqrt{1+\lambda^2}). \quad . \quad . \quad (19)$$

When λ is given, we can define by (18) the greatest value of λ' which is compatible with the conditions of equilibrium. The equation (19) then gives the corresponding value of E . If, for instance, $\lambda=0$, then

$$\frac{P}{2} = -\frac{1}{3}, \quad \lambda' = 0, \quad E = 0;$$

if $\lambda=1$, then

$$\frac{P}{2} = -0.2464, \quad \lambda' = 0.699, \quad E = 0.2608;$$

if $\lambda=2.5$, then

$$\frac{P}{2} = -0.22169, \quad \lambda' = 1.732, \quad E = 0.628.$$

Every value of λ' smaller than those here calculated must, therefore, also satisfy the conditions of equilibrium.

Gothenburgh, June 12, 1860.

XII. *On a Theorem relating to the Attraction of the Ellipse.*

By G. R. DAHLANDER*.

CERTAIN investigations relative to the attraction of the ellipse have led me to a theorem analogous to Ivory's well-known theorem concerning the attraction of the ellipsoid, which I will here communicate, as, so far as I am aware, it has never before been remarked.

It is supposed that from every element of the surface of the ellipse emanates an attracting force proportional to the area of the element, and varying according to a function $F(r)$ of the distance of the element from the attracted point, which is supposed to be in the plane of the ellipse.

Let the equation of the ellipse be $\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$, and let A_x and A_y denote the components of attraction parallel to the x and y axes, and let α and β be the coordinates of the attracted point. Then

$$A_x = \mu \int_{-b}^b dy \int_{-\frac{a}{b}\sqrt{b^2-y^2}}^{\frac{a}{b}\sqrt{b^2-y^2}} dx \frac{\alpha-x}{r} F(r),$$

in which expression μ is a constant quantity, and

$$r = \sqrt{(\alpha-x)^2 + (\beta-y)^2}.$$

The result of the integration according to x can be expressed in the form

$$A_x = \mu \int_{-b}^b dy (\phi(r_1) - \phi(r_2)),$$

where $\phi(r) = \int dx \frac{\alpha-x}{r} F(r)$, and r_1 and r_2 denote the values of r

corresponding to the values $\frac{a}{b}\sqrt{b^2-y^2}$ and $-\frac{a}{b}\sqrt{b^2-y^2}$ of x .

If these limiting values of x be denoted by x_1 and $-x_1$, it follows that

$$r_1 = \sqrt{(\alpha-x_1)^2 + (\beta-y)^2} \text{ and } r_2 = \sqrt{(\alpha+x_1)^2 + (\beta-y)^2}.$$

If we now consider another ellipse, $\frac{x'^2}{a'^2} + \frac{y'^2}{b'^2} = 1$, confocal with the former, and passing through the point (α, β) , we find in the same way that the attraction which it exercises on the point (α', β') has a component parallel to the x axis,

$$A'_x = \mu \int_{-b'}^{b'} dy' (\phi(R_1) - \phi(R_2)),$$

* Communicated by the Author.

when

$R_1 = \sqrt{(\alpha' - x'_1)^2 + (\beta' - y')^2}$ and $R_2 = \sqrt{(\alpha' + x'_1)^2 + (\beta' - y')^2}$,
 x'_1 denoting the expression $\frac{a'}{b'} \sqrt{b'^2 - y'^2}$.

In order to compare A_x and A'_x with each other, we can transform the variables within the integral signs. If we take $x = a \sin p$, and $y = b \cos p$, we obtain

$$A_x = \mu b \int_0^\pi dp \cdot \sin \mu \cdot (\phi(r_1) - \phi(r_2)),$$

$$A'_x = \mu b' \int_0^\pi dp \cdot \sin p \cdot (\phi(R_1) - \phi(R_2)).$$

Supposing that the point (α', β') is in the periphery of the first ellipse, and that its coordinates are defined by the analogies $\alpha : \alpha' = a' : a$ and $\beta : \beta' = b' : b$, whereby (α, β) and (α', β') become so-called corresponding points, we get

$$R_1^2 - r_1^2 = (a^2 - a'^2) \left(\frac{\alpha'^2}{a^2} + \frac{\beta'^2}{b^2} - 1 \right),$$

when we substitute in the expressions for R_1 and r_1 the values of x_1, y, x'_1 and y' , observing at the same time that $a'^2 - a^2 = b'^2 - b^2$, because the ellipses are confocal. But as the coordinates of the point (α', β') must satisfy the equation $\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$, we have

$\frac{\alpha'^2}{a^2} + \frac{\beta'^2}{b^2} - 1 = 0$, and consequently $R_1 = r_1$. In like manner it can be shown that $R_2 = r_2$. The expressions within the integral signs in the values of the components of attraction are, consequently, equal, and we may thence conclude that

$$A_x : A'_x = b : b'.$$

In the same way we obtain

$$A_y : A'_y = a : a'.$$

Gothenburgh, June 12, 1860.

XIII. On the Transmission of Electrolysis across Glass.

By W. R. GROVE, Q. C., F.R.S*.

IF glass or an equally non-conducting substance be interposed between electrodes in an electrolyte, so that there be no liquid communication around the edges of the glass, it is hardly neces-

* Communicated by the Author, having been read at the Chemical Section of the British Association, June 28, 1860.

sary to say that, according to received opinions and experiments, no current passes and no electricity takes place. I was led by some theoretic considerations to think that this rule might not be without an exception; and the following experiment realized my views.

A Florence flask, well cleaned and dried, was filled two-thirds full of distilled water with a few drops of sulphuric acid added to it, and placed in an outer vessel containing similar acidulated water which reached to the same height as the liquid in the interior. A platinum wire was passed through a glass tube, one end of which was hermetically sealed to the platinum, but so as to allow a small portion of the wire (about $\frac{1}{20}$ th of an inch) to project; the other end of the wire was passed through a cork, and the cork fitted to the mouth of the flask; when the cork was introduced, the projecting end of the platinum wire was three-quarters of an inch below the surface of the interior liquid; a similar coated wire was dipped into the outer liquid, and this and the wire which passed through the cork were brought respectively into connexion with the extremities of the secondary coil of a Ruhmkorff apparatus. Upon the latter being excited by the battery, a stream of minute bubbles arose from both the platinum points, proving clearly that electrolysis took place notwithstanding the interposition of the glass. The portions of the flask above the liquid, both outside and inside, were perfectly dry, so that there could have been no communication of the current over the surface of the glass. This was further proved by removing the outer wire a short distance from the surface of the water, when sparks passed nearly equal in length to those which took place between wires from the terminals. As the outer wire was further removed, keeping it near the glass, the sparks passed along the surface of the latter for a short distance; and as it was further removed they ceased, thus showing conclusively that there was no passage of electricity over the upper and unwetted surface of the glass.

With distilled water unacidulated I could observe no effect of electrolysis.

With acidulated water and the same arrangement I could detect no signs of electrolysis when, instead of the Ruhmkorff coil, an intensity nitric acid battery of thirty cells was employed.

In the first experiment the evolution of gas gradually diminished, and ceased after about twenty minutes' experiment; but upon intercepting communication with the battery for ten minutes and then reconnecting it, the evolution took place again; or a recurrence of electrolysis could be produced by reversing the direction of the current.

When the flask, after twenty minutes' experiment, was removed

from the outer vessel and tapped, minute bubbles rose from the interior surface. When a tolerably thick test-tube was used instead of the Florence flask, a very slight effect of electrolysis could be detected; and when the outer wire was removed to a short distance from the surface, sparks passed, but not of half the length of those with the Florence flask. When, however, a large phial of somewhat greater contents than the Florence flask was used, the effects were the same as with the latter, showing, as I expected, that surface is an important element in the success of the experiment.

There seems little doubt from the above experiments that the electrolysis was effected by induction across the thin glass of the Florence flask; and its cessation after a time, and recurrence after interruption of the current, would seem to indicate something like a state of charge or polarization of the surface of the glass.

Whether the bubbles which arose from the interior surface of the glass were the effect of electrolytic action or mere air-bubbles, cannot be affirmed with certainty; but as there was distinct evolution from the platinum wires, the corresponding elements must have been either dissolved, evolved, or deposited somewhere, and the most probable place of evolution would be the surface of the glass. If this be so, the glass would act in effect just as an interposed plate of inoxidable metal, though the one acts by induction, the other by conduction.

The oxygen and hydrogen may, however, be spread over the surface of the glass without evolution in the form of gas; and when the glass is, so to speak, coated by the elements, decomposition would cease.

The quantity of gas in the above experiment was too small for analysis; and in all probability, could it have been examined, some mixed gas would have been found to have been eliminated from each electrode. When, however, there is a small interruption in the secondary circuit of Ruhmkorff's coil, producing a rapid succession of sparks, I have found that between electrodes in the same circuit true polar decomposition takes place, and a galvanometer is steadily deflected according to the direction of the current, while without such interruption the movements of its needle are most irregular. I therefore repeated the experiment of interposed glass with an interruption in the circuit, and found that electrolysis took place as before; and in this case I have little doubt was true polar decomposition.

Although I failed with thirty cells of the nitric acid battery, I should fully expect that a battery of very high intensity, such as the 500 cells nitric acid, or the water battery of Mr. Gassiot, would produce effects of electrolysis across glass without the use of the coil.

XIV. *Preliminary Notice on Thirteen Systems of Crystallization in the Mineral Kingdom, and their Optical Characters.* By Professor BREITHAUP, of Freiberg, Councillor of Mines*.

THE important discovery made by Councillor Jenzsch, that tourmaline had two optical axes (Poggendorff's *Annalen*, vol. cviii. p. 645), certainly delighted no one more than myself. Soon after reading that account it occurred to me that the same character must be found in those crystals of apatite and idocrase in which I had more than ten years since observed the asymmetrical condition of the pyramidal planes with respect to the base. Reich found the two optical axes beautifully distinct in a fine crystal of apatite from Ehrenfriedersdorf, *Pollachites haplotypicus* (see my 'Mineralogy, vol. ii. p. 277), having at one pole a smooth basal plane, and at the other a good cleavage plane. I estimate the angle that the two axes make at 6 degrees at the least.

The same applies, though in a less degree, to the apatite from Schwarzenstein in the Zillerthal in Tyrol, which, however, I have not measured; perhaps in this case the axes are only 2 degrees apart.

The apatite from St. Gothard, *Pollachites galacticus*, which I had measured, was polished, and I then found that it presented the same characters as the one from Schwarzenstein.

M. Lingke, the optician to the University, was kind enough to cut and polish for me two plates of the green idocrase from Piedmont (*Idocrasius calaminus*, B. M. p. 652); and I found two axes in this also very plainly evinced.

When I had obtained these results, I received a letter from M. Jenzsch, saying that he also had found two optical axes in apatite and idocrase.

Col. von Kokscharoff has tried to set aside my measurements of the idocrase. He believes that I used imperfect crystals for these trials; but he is wrong; my measurements were not only most carefully made but repeated very often, and I selected the best crystals, viz. those from Piedmont, and I found the inclination to the base as follows:—

One plane	= 152° 55'
Two neighbouring planes . . .	= 142° 50'
And the fourth, opposite the first.	= 142° 47'

Col. von Kokscharoff says he found the angle only = 142° 46 $\frac{3}{4}$ ' in all four planes.

* Translated from the *Berg- und Hüttenmännische Zeitung*, by William Hustler, Esq., and communicated by Professor W. H. Miller, F.R.S. &c.

These differences are certainly the smallest and most difficult which I have had to determine, especially when the crystals present only one termination, and where perimetrical measurements are impossible.

It would have been more than wonderful, if one had wished for such a difference of angles, to have found them, especially in so many crystals, all of which had their symmetry destroyed in one and the same way; and this was certainly more than I either expected or sought for. Nevertheless I see no reason why some crystals of idocrase, which I have not examined, may not be found having their planes symmetrically arranged; but those individuals found by me to be asymmetrical, when transparent, have most certainly, without exception, two optical axes. *This much is certain, that tetragonal and hexagonal substances, whose pyramidal and rhombohedral planes are arranged in an absolutely symmetrical order, cannot have two optical axes.*

I discovered the peculiarities of idocrase in quite a different manner from that in which Col. von Kokscharoff did those of clinochlore; for I investigated these bodies without a previously formed opinion, and it was many years afterwards that the two optical axes of idocrase were made known. On the contrary, Col. von Kokscharoff first found in clinochlore with hexagonal base that the terminal planes were in symmetrical order; and after he had discovered that this mineral had two optical axes, he found by new measurements that, with the same hexagonal base, the terminal planes were asymmetrical and differed considerably from his former measurements. This second trial may, however, be considered very satisfactory. He will now see the wrong he did me in doubting the correctness of my measurements of idocrase, and also that the *symmetrical* construction of the pyramidal planes of those minerals which have two optical axes can no longer be reckoned on.

I have also examined many zircons in an unprejudiced manner, and found varying angles in different specimens; but the planes of a pyramid always had equal inclination to their polar edges, and again other equal inclinations to the planes of the prism. Clinochlore has been too rashly set down as hemirhombic; and if the hexagonal prism were found on it tomorrow, it would be again proclaimed to be hexagonal, which it most certainly is and must be. It resembles the above-mentioned apatites, in which, it is true, a difference in the angles of only 15 minutes is shown. It is evident that the figure of the base, or the angle of the prism, must in asymmetrical substances determine the system of crystallization to which they belong. The absence of the planes of the prism is not necessarily of importance; nobody, as far as I know, has observed the prism in tungstate of lime (Scheelite).

It may be asserted that not only clinocllore, but also the other micas (the *Asterites*, 'B. M.' p. 375) which have been believed to possess one optical axis, but which have indeed two weakly developed axes, and may therefore be said to have *nearly* one axis, belong to the hexagonal system. And now my measurements of the tourmalines, so long doubted, will be recognized as correct. I may here remark that the angles I gave as belonging to the red tourmaline from Siberia, may require some correction, owing to the crystals examined not being well fitted for the purpose. But for the other tourmalines I had always good, sometimes first-rate crystals to operate on; moreover, occasionally differences of more than 30 minutes appeared in the inclination of the rhombohedral planes. Why have not these measurements been repeated by others, that they might be confirmed or rejected? Why has the incorrect assumption been retained, that the primary form of all tourmalines is a symmetrical rhombohedron? Measurements are possible with a hand goniometer to a quarter of a degree when the crystals are good. Truly, next to want of truth, indolence is the greatest foe to the advance of science.

The essential crystallographical varieties extend still further. Many years ago I observed that the four planes of a crystal of anatase had at one pole a fourfold inclination towards the tetragonal base; a learned friend at that time induced me to believe that I had supposed a plane to be the basal one, which was in fact only very obtusely pyramidal. Unfortunately I gave in—and ceased my measurements too soon, for I had found important differences. I have since lost the notes I took at that time. The mighty architect of the universe can have forgotten no law in the construction of the "crystal world" which would tend to the completeness and harmony of the whole.

I have often repeated these observations, and can now say with certainty that we find in anatase a fourfold inclination of the pyramidal planes to the base, which differ 34 minutes altogether, and form a tetraplohedron. These observations, however, are not yet terminated, and must be often repeated to ensure perfect accuracy. On this account anatase also must have two optical axes. Unfortunately a crystal of anatase which I sent to be cut and polished, in order to test this fact, broke during the operation. Further, I believe I may say from carefully conducted investigation, that the tungstates of lime (*Pyramidites hystaticus* and *Pyramidites macrotypicus*, 'Breithaupt's Mineralogy,' pp. 266, 268) have also an asymmetrical arrangement of their pyramidal planes, and will be found to have two optical axes. The following hexagonal minerals, from the analogy of careful observations, must be asymmetrical, and those which are transparent have two optical axes.

First, diopase; and I must say at once that its primary form will be found to consist of two-thirds of a rhombohedron and one-third of a rhombohedron: I estimated this myself some time ago, but only measured the angles of one set of polar edges. Haydenite ought to have the same character. It is classed as a chabasite, and so it appears to be at the first glance; but according to Levy, the inclination of its rhombohedral planes to the polar edges differs by some degrees. It is very probable that it is hexagonal, and that it has a rhombohedral-like "diplohedron" or "triplohedron" for its primary form. The late M. Schüler measured this mineral in Freiberg, and found that the polar edges had three angles. Schüler wished to publish this fact, but I do not believe that he did so.

Perhaps other chabasites are asymmetrical and have two optical axes. It is very probable that an asymmetrical arrangement of the primary pyramidal planes exists in magnetic pyrites. I have no other ground for believing this, than the fact of its magnetic character.

The Museum at Freiberg is indebted to M. Müller for a large crystal of magnetic pyrites from Norway; it is a prism more than an inch long, and has a basal plane an inch broad. This crystal has a most distinct magnetic axis, which does not, as I expected, run parallel to the principal axis, but is nearly or quite horizontal, and is indeed perpendicular, or nearly so, to two parallel planes of the prism. It may be proved hereafter that the magnetic and crystallographical axes harmonize. When we know better the optical characters of the various species into which idocrase, tourmaline, apatite, titanites and other minerals, according to my views, ought to be crystallographically divided, the angles which the two optical axes form will assist in determining and arranging them. Of the apatites which I have been able to measure exactly, *Pollachites galacticus* and *Pollachites haplotypicus* differ greatly.

Tesseral minerals also show particular laws. It is known that I found *quite* constant deviations in melanite, sp. gr. = 3.777 (*Granatus melanites*, B. M., p. 637), and in almandine (*Granatus almandinus*, B. M., p. 644), sp. gr. = 4.119; so that the icositetrahedron (the leucite form) is not a simple figure, but a combination of an obtuse tetragonal and an acute ditetragonal pyramid. According to the measurements, the inclination of sixteen principal edges gave the same angle = $131^{\circ} 48'$; the same edges, reckoned according to the formula $\frac{1}{2} J$, would give $131^{\circ} 48' 36''$; but the eight remaining edges, on two angles composed of four edges, lying diametrically opposite to each other, and, again, equal among themselves, gave the angle = $131^{\circ} 54'$, or a deviation of 6 minutes. The tetragonal pyramid is thus much more obtuse

than it would be if it represented the derivation from $\frac{1}{2}$ J (viz. by 6'). This obtuser pyramid, which I designate as P, is the especial primary form, while D, the rhombic dodecahedron, remains as the general primary form of the garnets. I found a greater difference in the inclination of the grossular garnet (*Granatus grossularis*, B. M., p. 635), but, owing to the want of a perfect crystal, I could not properly estimate the variation.

I may ask, who has ever taken the trouble to centre and measure a garnet crystal twenty-four times along the principal edges? Has not the labour of centring and measuring the crystals of tourmaline three times according to their polar edges been avoided?

Now that my measurements have so clearly proved those newly discovered optical appearances which are above described, I conclude that the tesseral symmetry of the garnet crystals above described is also destroyed, and that as a *tetragonal axis appears to be the principal one, it must also be an optical axis*. I gave utterance to these views with the greatest confidence at the sitting of the Freiberg Mining Union on the 17th of January last.

As melanite is opaque, I tried no experiments on it; but as M. Reich and I were examining some polished garnets on the 22nd of January in Werner's Museum, I was fortunate enough to find a specimen which showed one optical axis perfectly and beautifully. This specimen had a sp. gr. = 4.152 (nearly that of the crystals that were measured). We convinced ourselves, however, that there are red garnets which have no optical axis, —for example, that garnet which has the sp. gr. = 4.20 to 4.27, and which, according to Prof. Rammelsberg, is so rich in protoxide of manganese.

The beautiful hyacinth-red garnet from the granite druses of the island of Elba belongs to this class, and has the same optical character. This, the heaviest of the garnets, must be separated from almandine, and called *Granatus manganosus*: one must not be astonished that species of essentially different character appear in one genus of an old system of crystallization. We have only to remember the feldspars, where adularia and pegmatolite are monoclinohedric, and tetartine, Labrador, and pericline are triclinohedric. Who would have believed that at least one of the well-known topaz family is hemihedral? and yet it is so. I am much indebted to Dr. Krantz for sending me transparent garnets from Piedmont and the East Indies. I found one optical axis in Hessonite from Ala in Piedmont.

It now remained to determine whether the optical coincided with the tetragonal axis in those garnets which possessed one optical axis. For this purpose I had a cube cut by M. Lingke out of a crystal of Hessonite, having its planes parallel respect-

ively to the three planes mutually at right angles to each other, in which lie the twenty-four edges of the crystals before mentioned; and I found that one optical axis was plainly visible, in a direction perpendicular to a hexahedral plane. The cutting of the crystal was not quite perfect, and therefore the point of intersection was not very clear; but the double refraction was unquestionable. I am now having a similar cube cut out of a crystal of almandine.

It has long been known that I discovered a particular law in the crystallization of iron pyrites and cobalt-glance, by which the pentagonal dodecahedron may be disjoined and formed into a combination of two rhombohedrons, while the hexahedron and octahedron retain their peculiarities. Naturally all forms with two- and threefold edges must behave in the same way, and be capable of being disjoined and formed into figures with one axis. I never found even two of those truncations, which are placed obliquely on the edges of the cube, to have a like inclination to the hexahedral planes.

The crystals of iron pyrites used in these investigations were from Kongsberg in Norway (with calc-spar), from Traversella, from Schemnitz in Hungary (in druses with galena, and blende), and from Mautern in Styria. The difference in the inclination of the planes of a supposed pentagonal dodecahedron is only 4 minutes, as shown in the above-mentioned crystals; but in the cobalt-glance from Skutterud in Norway, and from Tunaberg in Sweden, it is above 8 minutes. These facts have up to this time been ignored in mineralogical treatises, but have never been contradicted.

M. Websky, a pupil of mine, has confirmed them; he found a difference of from 5 to 10 minutes, and presented his observations to me, which I take this opportunity of acknowledging.

The behaviour of the last-named minerals so far resembles the garnets, that if you place the crystal so as to have the axis of an acute rhombohedron perpendicular, it will be exactly represented by the formula $\frac{1}{2}J$, while the obtuse rhombohedron is still more obtuse than it would be if represented by this formula. These pyrites have also one of the four hexagonal axes as the principal axis. The hexahedron will remain as the general primary form of this genus, which I call *Marcasites*, and to which all the pyrites belong which crystallize in the tesseral form. But the obtuse rhombohedron designated for the future as R, will be an especial primary form for those species which do not possess a pentagonal dodecahedron, but which are formed by the combination of two rhombohedrons.

It may happen, and I think it is probable, that pentagonal dodecahedrons may exist in other species, perhaps in *Gersdorffite*, and I fully expect it will be found in *Hauerite*, which, however, does not belong to that genus. In this case an especial primary form does not exist.

If iron pyrites and cobaltine could be rendered transparent, they would be found to have one optical axis that coincides with their hexagonal axis.

I am here reminded that Sir David Brewster recognized boracite forty-one years ago, as having one optical axis which coincided with a hexagonal one.

It appears very likely that the form of boracite, which has up to this time been regarded as a trigonal dodecahedron, may not be a simple form, but a combination of figures with one axis. I stated at the above-mentioned meeting of the 17th of January, that this was my belief; and the measurements which have been made since that time on some pretty clear crystals, have proved that I was right.

At first the cube, the pentagonal dodecahedron, and the tetrahedron gave their proper angles; but I afterwards found that though three of the planes on the three-edged angles gave the proper inclination towards the hexahedral planes for the trigonal dodecahedron, and exactly represented the formula $\frac{1}{2}J$, the fourth differed materially.

The first three gave the angle $144^{\circ} 44'$, reckoned according to the formula $\frac{1}{2}J$; it ought to have been $144^{\circ} 44' 8''$; but the fourth gave $144^{\circ} 17'$, a difference of 27 minutes; the crystals presented no difficulty to the measurer.

These figures, therefore, when placed upright on their hexagonal axis, resolve themselves into an acute hemimorphous scalenohedron, and into an obtuse hemimorphous rhombohedron, which we will designate as R; this last form is again more obtuse than if it had been deduced from the formula $\frac{1}{2}J$. The figures of the hemimorphous scalenohedron, and of the trigonal prism (the last reminds one of the same form in *tourmaline*), are placed in such a manner round the three hexagonal poles as the formula requires, and form $\frac{3}{24} = \frac{1}{8}$ of the planes belonging to a deltoid icositetrahedron. The hemimorphous R, on the other hand, gives $\frac{3}{24} = \frac{1}{8}$ of the planes of another deltoid icositetrahedron. The figures must in this case be held in the same way as they were before examined; and the hexahedron or the rhombohedron of the rhombic dodecahedron can now be taken as

the general primary form, and the R as the especial primary form.

The crystallographer must in future take a hexagonal axis as the principal one in boracite, as well as in the above-mentioned pyrites. The value of the form R is easily calculated; from the angles it contains, it appears that the inclination of the planes to the principal axis is $70^{\circ} 59'$. If, then, we use the formula $\frac{\frac{1}{2}J}{2} = 1$ to express the principal axis of such a rhombohedron, we obtain, from $\frac{39}{40}$ of the planes and the angle they form with the chief axis, the angle $70^{\circ} 58' 10''$; the other angle we found $= 144^{\circ} 17'$, which, according to calculation, $= 144^{\circ} 17' 2''$. If we deduce R from the rhombohedron of the rhombic dodecahedron, we get the coefficient $\frac{39}{80}$, and from the cube $\frac{39}{60}$.

Further, the inclination of the planes to the polar edges of R is found to be $147^{\circ} 12' 46''$;

if calculated after $\frac{\frac{1}{2}J}{2}, = 146^{\circ} 26' 33''$;

$46' 13''$, which is an important deviation, and could easily be found with the hand-goniometer on a crystal the size of the tip of one's finger.

It is on account of this R that boracite has one optical and one crystallographical axis; and thus, owing to prejudiced opinions, boracite has been considered isometrical for forty-one years.

If we wish to carry our comparisons between the tesseral system and the symmetrical tetragonal and hexagonal systems further, we find some of the minor divisions wanting. The tetragonal garnets represent the holohedral division of the tetragonal systems.

May not a mineral be found also of a tetragonal character which has hitherto been called semitesseral? It seems very probable to me that among those minerals which I have called Clinoedrites, viz. Kupferblende, Tennantite, Fahlerz, Schwarzerz, and crystallized Weisgültigerz (from Freiberg), some species may be found having other symmetrical forms than those which they are generally supposed to possess. Pharmakosiderite, helvin and eulytine must have their two-edged forms investigated; and then, I fancy, the missing links will be found. Further, were it possible to construct two hexagonal pyramids out of that tetrakis-hexahedron $\frac{1}{2}J$ (which is the holohedral form of the pentagonal dodecahedron), when placed upright on a hexagonal axis (and this might be occasioned by the necessary difference in the angles), then an analogy would be established between the hexagonalized tesseral system and the holohedral division of the symmetrical hexagonal system. Perowskite, and perhaps fluor-

spar, should be examined for this purpose. Is it not possible that different species of this mineral may be discovered, when we reflect that specimens from more than a hundred localities may be found in collections? I have found the sp. gr. to vary from 3.017 to 3.324. Is it not possible that a fluor-spar might be discovered having one optical axis? Who can boast of having undertaken optical or crystallographical researches on this mineral? If we estimate the number of the systems of crystallization by the important mathematical differences which we have here described, and which are in part only slightly marked, in the same way that the old rhombic system has been divided into four systems, we must form thirteen systems of crystallization, which are divided into four groups after the old ones:—

1st Group. *Tesseral System.*

- A. Isometrical tesseral, without optical axis. Spinnelle.
- B. Anisometrical tesseral, with one optical axis.
 - 1. Tetragonalized tesseral. Some of the garnets.
 - 2. Hexagonalized tesseral. Boracite, iron pyrites, cobaltine.

2nd Group. *Tetragonal System.*

- A. Symmetrical tetragonal, one optical axis. Zircon, rutile.
- B. Asymmetrical tetragonal, two optical axes.
 - 1. Monasymmetrical tetragonal. Idocrase.
 - 2. Diasymmetrical tetragonal. Anatase.

3rd Group. *Hexagonal System.*

- A. Symmetrical hexagonal, with one optical axis. Carbonates of lime, &c., quartz, beryl.
- B. Asymmetrical hexagonal, two optical axes.
 - 1. Monasymmetrical hexagonal. Some apatites, clinochlore, and other micas, *Turmalinus amphibolicus*, *Turmalinus ferrosus* (B. M. pp. 704, 706).
 - 2. Diasymmetrical hexagonal. *Turmalinus hystaticus*, *Turmalinus dichromaticus*, *Turmalinus medius*, *Turmalinus calaminus* (B. M. pp. 698, 700, 703, 704).

4th Group. *Heterogonal?, or Rhombic System.*

- A. Holoprismatic.
 - 1. Symmetrical heterogonal. Anhydrite, Arragonite, cymophane.
 - 2. Monasymmetrical heterogonal. Sulphate of iron, blue carbonate of copper, epidote, pyroxene, amphibole.
- B. Hemiprismatic.
 - 1. Diasymmetrical heterogonal. Adularia, pegmatolite.
 - 2. Triasymmetrical heterogonal. Pericline, microcline, tetartine, axinite.

In each of these four groups the determining forms are continued as before.

In the first, the cube, octahedron (with the tetrahedron) and the rhombic dodecahedron.

In the second, the basal pinacoid and the two prisms differing 30° from each other.

In the fourth, the pinacoids of the base and of the macro- and brachy-diagonals.

Moreover, I take each base as being horizontal and each prism as vertical.

The asymmetrical forms are arranged according to the length of their various axes.

Other circumstances, which are well enough understood, must be taken into account for the further division of one-axed forms.

All this and much more will shortly appear in a forthcoming work.

Especial notice will be taken of the analogy between the crystallographical, optical, electrical, and magnetic phenomena; and Councillor Reich has kindly promised a valuable paper on the last-mentioned subject.

Six new systems of crystallization (*i. e.* the sections B of the first three groups) are thus to be added to those already known; and by this means all the systems are brought nearer to each other. No doubtful conclusions have been formed, for they are all founded on ascertained facts. A person must have some self-confidence who has used scientific means for assisting him in his researches for more than forty years, and who during that period has measured between 12,000 and 13,000 angles with the reflective goniometer, and has also determined the specific gravity in more than 4000 instances, and who has often had to deal with inferior specimens.

These new systems will perhaps have to contend with a good deal of opposition; but I am convinced that their correctness will be established by investigation.

Feb. 16th, 1860.

I have been enabled this day to examine a plate of diopase one millimetre thick, cut from a transparent crystal perpendicular to the principal axis, with Amici's polarizing microscope, which has already been productive of so much benefit; and to my great satisfaction the plate showed two optical axes in a beautiful manner. I estimate the angle formed by the two optical axes at 4° . Thus the rhombohedron cannot be the primary form of diopase; it must be either a rhombohedron-like diplohedron, or triplohedron. I hope by measurements to determine this satisfactorily.

The same analogy which led me to suppose that diopase had two optical axes, has also reference to the tungstates of lime, *Pyramidites hystaticus* and *Pyramidites macrotypicus* (B. M. pp. 266 to 260); and I most certainly believe that the asymmetrical condition of their planes will be discovered; and if transparent crystals can be obtained, they will be found to have two optical axes.

The translator has had a letter from Professor Breithaupt, wishing him to add the following remarks to this paper.

“The tungstates of lime show two optical axes beautifully. I have measured Hessonite and found the variation to be 12 minutes, while in almandine and melanite it was only 6 minutes.

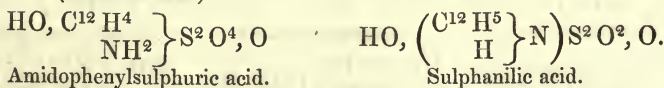
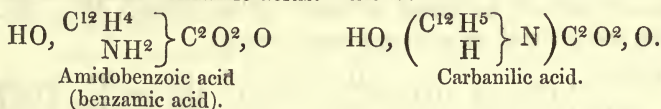
“Further, mellite or honeystone shows two optical axes, and is also very decidedly asymmetrical in the same way as idocrase, though with a greater difference in its angles.

“Finally, I have found a calc-spar or carbonate of lime with one optical axis and of asymmetrical form.”

XV. Chemical Notices from Foreign Journals. By E. ATKINSON, Ph.D., F.C.S., Teacher of Physical Science in Cheltenham College.

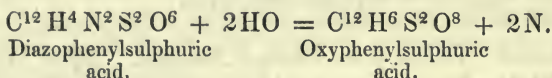
[Continued from p. 50.]

ACCORDING to Laurent, sulphanilic acid is obtained by the reducing action of sulphide of ammonium on nitrosulphophenylic acid; and this acid is identical with the acid obtained by the action of anhydrous sulphuric acid on aniline. Kolbe is of opinion, on the contrary, that they are only isomeric, and that the same relation obtains between them as between the isomeric carbanilic and benzamic acids. Thus:

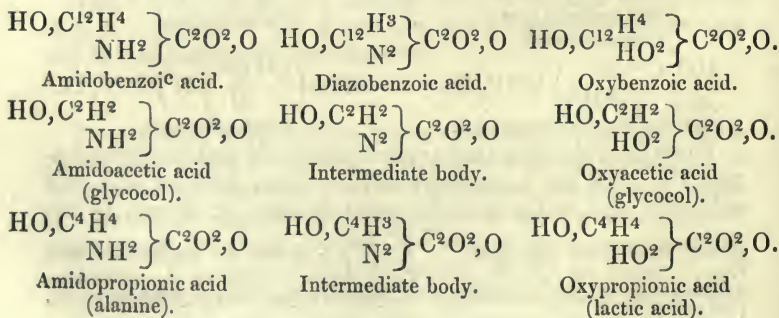


These formulæ show that amidobenzoic acid and amidosulphophenylic acids are respectively derived from carbonic acid, and sulphuric acid by the substitution of an atom of oxygen by the group amidized phenyle, $\left. \begin{array}{c} \text{C}^{12} \text{H}^4 \\ \text{NH}_2 \end{array} \right\}$; while in carbanilic and sulphanilic acids this atom of oxygen is replaced by phenylamide, $\left. \begin{array}{c} \text{C}^{12} \text{H}^5 \\ \text{H} \end{array} \right\} \text{N}$, that is, amide in which an atom of hydrogen is replaced by phenyle.

Schmidt*, in a preliminary notice, states that experiments he has made show that these acids are not identical. At the same time he describes the action of nitrous acid on sulphanilic acid. By this action a new body is formed which crystallizes in needle-shaped crystals. It has the formula $C^{12}H^4N^2S^2O^6$, and may be regarded as phenylsulphuric acid in the phenyle of which two atoms of hydrogen are replaced by two atoms of nitrogen: thus, $HO, \left. \begin{matrix} C^{12}H^3 \\ N^2 \end{matrix} \right\} S^2O^4, O$. Schmidt names it *diazosulphophenylic acid*. It has acid properties, but is very unstable. When its aqueous solution is heated to 60° , a decomposition ensues accompanied by a copious disengagement of pure nitrogen. The acid liquid now contains a new acid, *oxyphenylsulphuric acid*, $HO, \left. \begin{matrix} C^{12}H^4 \\ HO^2 \end{matrix} \right\} S^2O^4, O$, which is phenylsulphuric acid in which an atom of hydrogen in the phenyle is replaced by peroxide of hydrogen. The formation may be thus expressed:—



Oxyphenylsulphuric acid corresponds to oxybenzoic acid, and diazosulphophenylic acid to diazobenzoic acid, the remarkable body obtained by Griess† by the action of nitrous acid on alcoholic solution of benzamic acid. Schmidt considers that in all cases in which amidized acids, such as amidoacetic acid, are converted into oxyacids, intermediate bodies, of which diazobenzoic acid is a representative, are first formed. Hence we might anticipate that between glycol and glycolic acid, and between alanine and leucic acid, intermediate bodies corresponding to diazobenzoic acid will be found to exist. A comparison of the formulæ will show these relations more clearly.



* Liebig's *Annalen*, October 1859.

† Phil. Mag. vol. xvii. p. 371.

In Peru and in other South American countries the coca leaf (*Erythroxylon coca*) has long been extensively used as a narcotic. As to its effects, accounts differ much. According to Von Tschudi*, its use in moderate quantities is that of a beneficial stimulant, and enables persons taking it to do without food for a long time, and to bear great fatigue. But its immoderate employment produces effects analogous to those caused by excessive use of opium. The peculiar physiological effects have been ascribed to the presence of a narcotic principle, the existence of which, however, has not been established.

The plant (a supply of which was obtained through the Austrian surveying frigate 'Novara') has been investigated by Wöhler† assisted by Niemann, who have established the existence in it of a peculiar crystallized organic base, to which the name *cocaine* has been given. The investigation is not completed; the formula is not fixed, nor has the chief point been settled, whether the peculiar physiological effects are attributable to this base. To obtain the base, the leaves were digested with alcohol containing some sulphuric acid, the solution pressed and mixed with hydrate of lime, by which a wax and part of the chlorophyll were precipitated. The filtered alkaline solution was neutralized with sulphuric acid, and the alcohol distilled off in the water-bath. The addition of water to the residue caused the precipitation of a semifluid mass which contained the remainder of the chlorophyll. The supernatant liquid contained sulphate of cocaine, from which the base was precipitated on the addition of carbonate of soda. By treating this precipitate with ether, the *cocaine* was dissolved out, and remained on evaporation of the ether as a yellowish mass, which afterwards crystallizes. By repeated treatment with alcohol it is obtained pure and colourless.

Cocaine crystallizes in small, white, colourless and inodorous prisms. It is little soluble in water, more so in alcohol and in ether. It has a bitter taste, and exerts a peculiar action on the tongue; the parts touched appear quite without feeling. It melts at 98°, and solidifies crystalline. At a higher temperature it decomposes with formation of ammoniacal compounds. Cocaine is strongly alkaline; it completely neutralizes acids, but the salts remain amorphous for some time before crystallizing.

Cocaine most resembles atropine; but it appears to differ in composition, and in some properties. The gold salts of both bases are precipitated from their cold hydrochloric acid solutions on the addition of chloride of gold as amorphous masses, and from their heated solutions in crystalline laminæ. But the

* Johnson's Chemistry of Common Life, chap. xx.

† *Göttinger Nachrichten*, No. 10, 1860.

cocaine compound is distinguished by giving off benzoic acid when it is distilled—a fact in which will probably be found the key to its composition. Unlike atropine, cocaine does not act on the pupil.

Fremy has published* an important investigation on the green colouring matter of leaves. He has found that it consists of a blue and yellow principle, which he has succeeded in isolating.

The colouring matter is contained in the green oil which is extracted by alcohol from leaves. This oil may provisionally be termed chlorophyll, but it contains several other substances which render the separation of the colouring matter difficult. The blue and yellow colouring principle have a different affinity for hydrate of alumina. When a strong alcoholic solution of chlorophyll was digested with hydrate of alumina, no alteration took place; but by adding a small quantity of water, a dark-green, almost blue precipitate was obtained, and the alcohol solution was of a yellow colour. When a considerable quantity of water was added, the precipitate had a colour like that of the ordinary colouring matter. Although this experiment effected some separation of the two colouring matters, the separation could not be carried further by its means.

Fremy next tried the action of different neutral solvents on the combination of alumina with the green colouring matter. He found that some, such as bisulphide of carbon, dissolved in preference the combination of yellow colouring matter and alumina; others, such as ether, alcohol, turpentine, dissolved out the green matter. By employing successively these different solvents, after the use of bisulphide of carbon, he succeeded in obtaining lakes of different shades, but was not able to carry the separation further.

The usual reducing agents, which change other colours, do not affect chlorophyll. But by the action of bases this body is converted into a yellow colour, which forms with alumina a beautiful yellow lake, soluble in neutral solvents, such as ether, alcohol, bisulphide of carbon. By acting on the solution of this body with acids, especially hydrochloric acid, it was transformed into the original green. Now, assuming that the green thus formed was composed of blue and yellow colouring matters, the point was to separate these two bodies at the moment of their formation. This must be done by the simultaneous use of solvents which act differently on the two colouring matters. Such a solvent is a mixture of hydrochloric acid and ether; and it was used as follows:—

Two parts of ether and one part of hydrochloric acid, diluted

* *Comptes Rendus*, February 27, 1860.

with a little water, were shaken in a stoppered bottle for some time, so as to saturate the ether with hydrochloric acid. When the solution formed by the decolorization of the chlorophyll by bases was shaken with this solution, a remarkable change took place; the ether retained the yellow matter, and the hydrochloric acid the blue colouring principle. The two colours were thus isolated; but if now alcohol in excess was added, which dissolves both the yellow and green colouring matters and their solvents, the solution became of the original green tint.

To the yellow matter soluble in ether, Fremy gives the name *phylloxanthine*, and to the blue colouring matter the name *phyllocyanine*. To the other yellow body which results from the change of phyllocyanine, he gives the name *phylloxantheine*.

The blue and yellow colouring matters may be obtained directly from chlorophyll by adding the ether and acid mixture directly to the alcoholic extract of the leaves. The green first becomes brown and is then resolved into phyllocyanine, which dissolves in the acid, and phylloxanthine, which dissolves in the ether. This interesting experiment may also be made directly with the leaves.

Fremy found that the yellow colouring matter formed in the young shoots is the same as that resulting from the decomposition of chlorophyll. It may be extracted by alcohol, and partially resolved into yellow and a little blue colour by means of hydrochloric acid and ether. Leaves which become yellow in autumn, then only contain phylloxanthine.

XVI. *Note regarding Mr. Ponton's Paper, "On certain Laws of Chromatic Dispersion."* By BALFOUR STEWART.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

HAVING perused a paper "On certain Laws of Chromatic Dispersion" which appeared in your Journal of last March as a communication from Mr. Ponton, I feel the following difficulty in recognizing as additions to our knowledge certain laws which this author claims to have discovered.

To adopt Mr. Ponton's notation, let U be the length in free æther of the undulation corresponding to one of Fraunhofer's seven fixed lines, and let u denote the wave-length of the same line within a given medium after refraction. Then the relation between U and u may be expressed by the equation

$$\epsilon(u + a \pm x) = U,$$

where the quantities ϵ and a are constant for the same medium and temperature, being the same for all undulations; while, on

the other hand, the quantity x is peculiar to each wave, the medium and temperature remaining the same.

Adopting, therefore, the above equation, and applying it to the seven lines B, C, D, E, F, G, H, we obtain, in Mr. Ponton's notation, the following seven equations:—

$$\epsilon(b + a \pm b_x) = B,$$

$$\epsilon(c + a \pm c_x) = C,$$

$$\&c. \quad \&c. \quad \&c.$$

$$\epsilon(h + a \pm h_x) = H.$$

In these, b , c , &c., B, C, &c. are supposed to be determined by observation. We have thus remaining as unknown quantities ϵ , a , b_x , c_x , &c. (nine in all), which require to be determined.

In order to accomplish this, it is necessary to make two additional assumptions. These assumptions are quite arbitrary, and have no relation to physical science.

The first of these is made by Mr. Ponton in page 167, where he assumes as true the following equation:

$$\frac{(3B + 2C + D) - (F + 2G + 3H)}{(3b + 2c + d) - (f + 2g + 3h)} = \epsilon.$$

An inspection of the seven equations given above will show that this assumption implies that the following equality subsists:

$$\pm 3b_x \pm 2c_x \pm d_x = \pm 3h_x \pm 2g_x \pm f_x.$$

This equation, which is the immediate consequence of Mr. Ponton's first assumption, he seems to regard as the general expression for a law of nature, which he denominates the semelbis-ter law (see page 172). The second assumption is also made in page 167.

Calling $B + C + D + E + F + G + H = S,$

$$b + c + d + e + f + g + h = s,$$

it is assumed that

$$\frac{S - \epsilon s}{7\epsilon} = a.$$

An inspection of the same seven equations will show that this assumption implies that

$$b_x + c_x + d_x + e_x + f_x + g_x + h_x = 0.$$

This consequence of his second assumption appears also to be recognized by Mr. Ponton as a law of nature in page 168, where it is said, "In every case the motive energy gained by the one set of waves is exactly balanced by the loss sustained by the others; so that the sums of the positive and negative extrusions being each denoted by X, these two quantities are always equal."

... This, which may be called 'the law of equal transference,' is first to be recognized; and it is found to prevail in all media, whether regular or peculiar."

It thus appears that Mr. Ponton regards "the law of equal transference" and "the semel-bis-ter law" as laws of nature; whereas I am unable to view them as anything else than the immediate results of two arbitrary assumptions which it was necessary for Mr. Ponton to make before he could determine nine unknown quantities by seven equations.

I am, Gentlemen,

Yours sincerely,

Kew Observatory,
June 1860.

BALFOUR STEWART.

XVII. *Sketch of a Theory of Transcendental Roots.*

By JAMES COCKLE, M.A., F.R.A.S., F.C.P.S. &c.*

IN these and other† pages I have, I believe, proved the hopelessness of solving a quintic by means of an Abelian sextic; at all events I have given formulæ which afford a ready test of any supposed solution. Solubility by radicals is, indeed, a characteristic of equations of the lower degrees, and of certain forms of those of the higher; but the conclusions of Abel and Sir William Rowan Hamilton indicate that it is not a property of equations in general. And if we would study the general problem of solution, we must introduce functions which are not in general algebraic, though in special cases they may be susceptible of algebraical expression. The introduction of such functions is facilitated by results with which Cardan, Tschirnhausen, and Mr. Jerrard have enriched science, and which manifest the possibility of reducing to the form

$$x^n - nx + (n-1)a = 0$$

any general equation of a degree lower than the sixth. The possibility of this transformation entails, save in the case of quadratics, that of the transformation

$$x^n - nax + a^n = 0$$

within the same limits. The non-symmetric and the symmetric forms alike disclose that the roots are functions of a single parameter; and, inasmuch as these forms are substantially as general

* Communicated by the Author.

† See my "Notes on the Higher Algebra" in the current (June 1860) Number of the 'Quarterly Journal of Pure and Applied Mathematics.' In a paper "On the Resolution of Quintics," which appears in the same Number, I have given some additional development to formulæ discussed in this Journal.

as the general equation, the roots of the latter are also functions of a single parameter, and that equation admits of the application of the processes which follow without any preliminary transformation. Retaining the above forms for the sake of brevity, let either of them indifferently be represented by

$$fx=0,$$

and either of their systems of roots by

$$\phi_1 a, \phi_2 a, \dots \phi_n a,$$

no assumption whatever being made as to ϕ , except its fundamental property of satisfying the condition

$$f\phi a=0$$

identically. The next step is to treat this equation by the calculus of functions, or rather (since the discoveries of M. Hermite and of M. Kronecker have shown that the roots of a quintic are expressible in terms of elliptic integrals) by the differential calculus, which, where the roots are obtainable in terms of integrals, ought to enable us to obtain them. In consequence of its identical nature, the successive derived equations

$$\frac{df\phi a}{da}=0, \quad \frac{d^2 f\phi a}{da^2}=0, \dots$$

are satisfied identically. Replace ϕa by x in each of these relations, and seek the form of ϕ . For the quadratic we have

$$x^2 - 2x + a = 0,$$

$$2x \frac{dx}{da} - 2 \frac{dx}{da} + 1 = 0;$$

and the elimination of x leads to

$$(4a-4) \left(\frac{dx}{da} \right)^2 + 1 = 0,$$

whence

$$\begin{aligned} \frac{dx}{da} &= \frac{1}{2\sqrt{1-a}}; \quad x = \int \frac{da}{2\sqrt{1-a}} + \text{const.}, \\ &= 1 - \sqrt{1-a} = \phi a, \end{aligned}$$

the ordinary solution. But here the radical flows from the integral expression. Again, for the cubic we have

$$x^3 - 3x + 2a = 0,$$

$$3x^2 \frac{dx}{da} - 3 \frac{dx}{da} + 2 = 0;$$

and, by the elimination of x ,

$$(a^2-1)\left(3\frac{dx}{da}\right)^3+9\frac{dx}{da}+2=0.$$

Multiplying this result into $\sqrt{1-a^2}$, and making

$$-3\sqrt{1-a^2}\cdot\frac{dx}{da}=\xi,$$

it becomes

$$\xi^3-3\xi+2\sqrt{1-a^2}=0,$$

which is of the same form as the given cubic. Hence, since

$$x=\phi a,$$

therefore

$$\xi=\phi\sqrt{1-a^2},$$

and

$$\frac{dx}{da}=-\frac{\phi\sqrt{1-a^2}}{3\sqrt{1-a^2}},$$

of which a corrected integral is

$$x=2\sin\left(\frac{2\pi+\sin^{-1}a}{3}\right),$$

which expresses the mean root of the cubic. And if we call

$$x_1=2\sin\left(\frac{\sin^{-1}a}{3}\right)=\phi_1a,$$

$$x_2=2\sin\left(\frac{2\pi+\sin^{-1}a}{3}\right)=\phi_2a,$$

$$x_3=2\sin\left(\frac{4\pi+\sin^{-1}a}{3}\right)=\phi_3a,$$

we have

$$\frac{d\phi_1a}{da}=-\frac{\phi_3\sqrt{1-a^2}}{3\sqrt{1-a^2}},$$

$$\frac{d\phi_2a}{da}=-\frac{\phi_2\sqrt{1-a^2}}{3\sqrt{1-a^2}},$$

$$\frac{d\phi_3a}{da}=-\frac{\phi_1\sqrt{1-a^2}}{3\sqrt{1-a^2}};$$

and the mean root alone satisfies all the conditions of the pro-

cess. Thus far I have used the non-symmetric form; but inasmuch as the symmetric one leads to

$$x = \phi a, \quad a = \phi x = \phi \phi a,$$

we have, in $\phi^2 a = a$, a relation which further limits the form of ϕ . Whichever form we employ, the first derived equation may be depressed to the second degree in x , and for the latter we find

$$x^{n-1} \frac{dx}{da} - a \frac{dx}{da} - x + a^{n-1} = 0,$$

or, reducing,

$$(n-2)a \frac{dx}{da} + \left(\frac{1}{x} \cdot \frac{dx}{da} - 1 \right) (x - a^{n-1}) = 0.$$

In treating of equations which involve more than one parameter, we must employ the differential equations

$$\frac{df}{da} da + \frac{df}{db} db + \dots = 0, \quad \frac{d^2 f}{da^2} da^2 + \frac{2d^2 f}{da db} da \cdot db + \dots = 0, \dots;$$

and however numerous be the symbols in the equation

$$f(v, w, \dots y, z) = 0,$$

we shall, provided that relation be symmetric, in the end be led to relations of the form

$$z = \psi y, \quad y = \psi z = \psi^2 y.$$

I mention this because it may be found desirable to study some canonical forms other than those pointed out by Mr. Jerrard. When a is greater than unity, logarithmic may replace the above circular functions, and, when the parameters are independent, the δ of the calculus of variations may replace d , and we shall have the identical equalities

$$f\phi(a, b, \dots) = 0, \quad \delta f\phi(a, b, \dots) = 0, \quad \delta^2 f\phi(a, b, \dots) = 0, \dots$$

I noticed the auxiliary condition $\phi^2 a = a$ in a footnote at p. 133 of this Journal for February 1846 (S. 3. vol. xxviii.). Subsidiary conditions are necessary, even in the foregoing discussion of a cubic, for suggesting or completely ascertaining the form of ϕ ; and it will be observed that

$$-\frac{dx}{da} = \phi \sqrt{1-a^2} \cdot \frac{d}{da} \left(\frac{C + \sin^{-1} a}{3} \right),$$

where C is a constant.

4 Pump Court, Temple, London,
June 30, 1860.

XVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 78.]

Jan. 12, 1860.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communications were read:—

“On the Movements of Liquid Metals and Electrolytes in the Voltaic Circuit.” By George Gore, Esq.

1. It has long been known that when a globule or layer of pure and clear mercury is placed upon a smooth non-metallic surface, a watch-glass for example, and covered to a small depth with a watery electrolyte, sulphuric acid in particular, and two terminal platinum wires from a voltaic battery are dipped into the electrolyte, one on each side of the globule, the mercury makes a movement towards the negative wire, and a rapid and *continuous stream* of the supernatant liquid flows from the negative to the positive electrode over the surface of the mercury, and back again by the sides of the containing vessel. Also that when a small drop of a watery electrolyte, especially sulphuric acid, is placed upon the surface of pure and dry mercury, the latter connected with the negative pole of a battery, and a platinum wire from the other pole momentarily immersed in the electrolyte, the drop of liquid is *suddenly repelled* and spreads over the surface of the mercury.

2. These phenomena have been examined by Herschel*, Erman, H. Davy, Runge, Pfaff, and others†, and some of the results have been recorded in the 1st volume of ‘Gmelin’s Handbook of Chemistry,’ page 486 (published by the Cavendish Society); but no definite cause of the movements seems to have been discovered. Herschel has, however, shown that the continuous movement of the supernatant liquid is unaffected by the approach of strong magnets, and that it is influenced by the chemical nature of the electrolyte; also that its direction is notably influenced by the presence of various metallic impurities in the mercury.

3. Being desirous of ascertaining the conditions under which the movements are produced, the relations of the phenomena to ordinary and recognized actions, and the more immediate cause or causes of the movements, I have undertaken the following experimental investigation.

4. In describing the experiments I shall have frequent occasion to speak of the continuous flow of the electrolyte, and of the sudden repulsion of drops of liquid already mentioned, and shall therefore speak of the former as the *continuous* action or movement, and of the latter as the *sudden* or *momentary* one. Also in speaking of the continuous motion, I shall call it *positive* flow or movement when the supernatant liquid proceeds from the positive wire towards the negative one, and *negative* flow, &c. when it passes in the opposite direction.

5. The usual method of manipulation I have adopted has been to

* “On certain Motions produced in Fluid Conductors when transmitting the Electric Current,” Phil. Trans. 1824.

† Draper has recorded some experiments of a similar kind.—Philosophical Magazine, S. 3. vol. xxvi. p. 185.

take a watch-glass of about 2 inches diameter and place in it by means of a small gutta-percha spoon capable of containing from 20 to 50 grains of mercury, a globule of that metal of about 30 grains weight, adding sufficient of the electrolyte to just cover or nearly cover the globule of metal, and sifting a few particles of finely powdered charcoal or asphaltum upon the surface of the liquid, to facilitate observation of the movements; next, using a Smee's battery of 22 pairs of plates 4 inches deep and $2\frac{1}{2}$ inches wide with terminal platinum wires, charged with one measure of oil of vitriol and 15 measures of water, I place the end of the negative wire in the liquid about $\frac{1}{4}$ th of an inch from the mercury, and then carefully immerse the end of the positive wire in the liquid on the opposite side of the globule, at a greater distance from the mercury than the negative wire in the case of an alkaline solution, and at a less distance in the case of an acid one, in order to prevent the mercury from touching the electrodes by its movement and thus vitiating the first and purest result. A polished oval space 2 inches long, $\frac{3}{4}$ ths of an inch wide, and $\frac{3}{8}$ ths of an inch deep, with a curved bottom, formed in a thick plate of glass and substituted for the watch-glass, did not admit of such satisfactory freedom of motion. In doubtful cases of movement, a small porcelain boat, such as is used in organic chemical analysis, was sometimes employed instead of the watch-glass; and in certain special experiments a V-tube was employed. In nearly all cases the mercury gradually became impure, and therefore fresh mercury was taken for each experiment.

A. Conditions of the Movements.

6. *Two* substances are always required in these experiments, with one alone the movements never occur.

7. To determine whether *both* the substances must be in a *liquid* state:—1st. A portion of mercury in a watch-glass was connected with the negative pole of a battery and covered with a flat piece of platinum foil; a drop of solution of sulphate of potash was placed upon the foil and the end of the positive wire dipped into it. No movement, either sudden or continuous, of the solution or mercury took place. On substituting for the foil a circular piece of filtering paper varnished all round its edge and covered with several drops of the solution of sulphate of potash, the sudden repulsions were produced readily, but were much less powerful than when the liquid was placed alone upon the mercury. 2nd. Two circular clean spaces, $\frac{1}{2}$ an inch wide, were scraped with a knife upon a horizontal plate of zinc; one of them was amalgamated with mercury and left covered with a very shallow layer of that metal, the other was also amalgamated, but the excess of mercury was wiped off; each of the spots was now covered with a shallow layer of a weak solution of sulphate of alumina, the zinc plate connected with the negative plate of the battery, and the end of the positive platinum wire dipped in succession into the supernatant portions of liquid; the solution above the thin layer of liquid mercury was powerfully repelled on making the contact, whilst that upon the other spot was unaffected. Similar results were obtained

with a solution of caustic potash, also with a plate of tin. 3rd. A portion of Newton's fusible alloy was melted under a layer $\frac{1}{8}$ th of an inch deep of a solution of chloride of zinc, and the ends of the platinum wires from the battery immersed in the supernatant liquid until the alloy cooled and solidified; the zinc solution flowed from the negative towards the positive wire as long as the surface of the alloy remained in the liquid state, and ceased to flow immediately the surface of the metal solidified. Also a drop of a strong solution of caustic potash placed upon the melted fusible alloy, the latter connected with the negative pole and the former with the positive pole, exhibited the usual momentary repulsions as long as the surface of the alloy remained fluid. I therefore conclude that *both* the substances must be in a *liquid* state.

8. To ascertain whether *both* the substances must be *conductors of electricity*:—1st. I formed melted globules of phosphorus in warm oil of vitriol, also in a hot mixture of one measure of distilled water and two measures of oil of vitriol, and immersed the wires in the usual manner, but no motion of the liquid occurred. 2nd. No movements were obtained with a globule of bromine under warm oil of vitriol; a large globule of bromine was placed in a porcelain boat, and dilute sulphuric acid added until the bromine was partly covered; the wires were then applied, but no movements took place. Also the addition of sulphur and of selenium to the bromine did not ensure a different effect. 3rd. With a large globule of selenium under fused chloride of zinc no motion was obtained. 4th. I made similar experiments with globules of chloroform, also of bisulphide of carbon in dilute sulphuric acid, but obtained no movements. 5th. No movements took place with globules of chloroform in a solution of caustic potash or of sulphate of alumina.

9. To determine whether one of the substances must be *metallic*:—1st. A definite layer of oil of vitriol was placed beneath a layer of distilled water weakly acidulated with sulphuric acid, and the terminal wires immersed in the upper liquid; no movements occurred at the boundary line of the two liquids. 2nd. A dense solution of cyanide of potassium was placed in a small glass beaker, a few particles of charcoal were sifted upon its surface, and a layer of aqueous ammonia $\frac{1}{2}$ an inch deep carefully poured upon it. A vertical diaphragm of thin sheet gutta percha was then fixed so as completely to divide the upper liquid into two equal parts; the vessel was placed in a strong light, and two horizontal platinum wire electrodes from 66 pairs of freshly-charged Smee's batteries immersed $\frac{1}{8}$ th of an inch deep in the liquid ammonia on each side of the diaphragm. A copious current of electricity circulated, but no movements of the liquids at their mutual boundary line could be detected. A small globule of mercury placed in the lower liquid at once produced evident signs of motion. One of the substances must therefore be a *metallic* conductor of electricity.

10. To ascertain whether the capability of producing these movements was a general property of metals and alloys when in the liquid state:—1st. Bismuth was fused beneath a layer of chloride of zinc; tin was also melted under a similar layer, and the terminal wires im-

mersed in the supernatant liquid ; a steady negative flow occurred in each case. 2nd. Cadmium was similarly treated under fused cyanide of potassium, and a positive flow obtained. 3rd. Cadmium, lead, Britannia-metal, and fusible metal were melted separately, small pieces of cyanide of potassium placed upon them and melted, the metal connected with the negative platinum wire, and the positive wire dipped into the melted cyanide ; positive repulsions took place with each metal on making contact. I conclude from these experiments that the power of rotating under the influence of an electrolytic current is a general property of metals and alloys when in a liquid state.

11. That the *mass* or *body* of the metal is not essential to the production of the movements, is evident from the fact that the movements have been readily obtained with *thin layers* of mercury upon amalgamated zinc (7) and copper plates.

12. I have endeavoured to obtain the movements without the presence of an electrolyte, by passing an electric current through a small globule of zinc fused upon the surface of bismuth, but the ready mingling of the melted metals, and their rapid oxidation, prevented a reliable experiment being made.

13. It has already been shown, in the instances of fused salts upon melted metals (10), that the presence of *water* is not a necessary condition of the phenomena.

14. The power of producing the movements is a general property of electrolytes as well as of liquid metals ; I have experimentally found it in the following classes of substances :—organic and inorganic acids ; water ; aqueous solutions of caustic alkalies* ; alkaline carbonates, bicarbonates, borates, hypophosphites, phosphates, sulphides, hyposulphites, sulphites, sulphates, bisulphates, iodides, bromides, chlorides, chlorates, nitrates, and silicates ; salts of alkaline earths and of alumina ; salts of tungsten, molybdenum, chromium, uranium, manganese, arsenic, and of the malleable heavy metals ; also with fused salts, aqueous solutions of organic salts, and solutions of salts in alcohol. The salts of tungsten, molybdenum, chromium, uranium, and manganese, generally gave the weakest and most variable results ; whilst sulphuric acid and solutions of alkaline cyanides yielded very strong and definite movements. In feeble cases of motion the globule of mercury should be placed in a narrow porcelain boat, and a strong solution of the substance added until the metal is only covered at its sides with the liquid ; and for still greater sensitiveness, the experiment of placing a drop of the liquid upon the surface of the mercury should be adopted.

15. The *mass* or *body* of the liquid is not essential to the movements ; mere films of solution adhering to the under surface of a circular disc of brass, brought into contact with mercury under the influence of a voltaic current, exhibited the phenomenon readily.

* Herschel found no movements with solutions of caustic alkalies (*Vide* Gmelin's Handbook, vol. i. page 490) ; I have readily obtained them with pure mercury in solutions of pure alkalies by using strong solutions and a powerful electric current, and placing only a small quantity of the liquid above the mercury so as to produce the maximum of effect. Alkaline solutions in general act much more feebly than acids.

16. To ascertain whether the current of electricity must pass from the electrolyte into the metal, or *vice versâ*:—1st. A layer of mercury was placed in a narrow glass beaker, upon it a shallow layer of chloroform, and above this, in one instance a dilute solution of sulphate of alumina; and in the other instance a solution of caustic potash, and the wires from the battery dipped into the upper liquid; no movements at either of the contiguous surfaces occurred. 2nd. Similar experiments were made, substituting in one case a definite layer of oil of vitriol with a very dilute solution of sulphuric acid above it, and in the other case a dense solution of chloride of zinc with a very dilute solution of the same salt above it, for the chloroform and its supernatant liquid; in each case only feeble movements in the usual direction at the surface of the mercury occurred; the weakness of the movements was probably a consequence of the increased distance of the electrodes from the mercury. 3rd. The lower part of a V-tube of half an inch bore was just filled with mercury, and a small quantity of solution of cyanide of potassium poured into each leg; on dipping the polar wires, one into the solution of each leg, the saline liquid rapidly flowed from the positive to the negative leg until it was $1\frac{1}{4}$ inch high in that limb. From these experiments I infer that the electric current must pass from the electrolyte into the metal, or *vice versâ*, and that the continuous movements are not results of any power *radiating* from the electrodes.

17. It is not essential that the electric current should pass both into and out of the metallic globule by the electrolyte; with a globule of mercury in rather strong sulphuric acid and either of the polar wires immersed in the acid, the other wire being in contact with mercury, the movements occurred: also with the negative wire touching a globule of mercury in a solution either of cyanide of potassium or strong caustic potash and the positive wire in the liquid, movements were readily obtained.

18. To ascertain whether the electrodes were essential to the movements, I placed a large globule of mercury in the middle part of a slightly bent horizontal glass tube, 20 inches long and $\frac{1}{2}$ an inch diameter, then filled the tube with a strong solution of cyanide of potassium, and immersed the polar wires a short distance in the liquid at each end; a strong positive flow of the solution over the surface of the mercury occurred, but no movements took place at the surfaces of the electrodes, except such as were produced by the evolution of gas. The electrodes evidently operate merely as conductors of the electricity, and are not, in an abstract sense, at all connected with the movements.

19. Herschel has shown that the approach of strong magnets has no effect on the motions (*vide* Gmelin's Handbook, vol. i. p. 490), and I have also found that the movements are not electro-magnetic. A watch-glass—containing in one instance a solution of cyanide of potassium, in a second instance a solution of hydrochlorate of ammonia, and in a third instance oil of vitriol,—was placed upon one of the poles of a vertical horse-shoe electro-magnet capable of sustaining about 100 pounds, and the end of a large soft-iron armature

which rested upon the other pole brought near the glass. The polar wires from the Smee's battery of twenty-two pairs were now immersed in the electrolyte on each side of the globule, and the magnet connected with a separate battery of large surface. The direction of flow of the electrolyte was instantly changed to a circular one all round the glass, and was reversed by reversing the polarity of the magnet. In each case the direction of motion of the electrolyte corresponded with that of the electric current beneath it; *i. e.* with a south pole beneath, the liquid moved in the same direction as the hands of a watch;—this circular motion was evidently a case of ordinary electromagnetic action, as it occurred equally well without the presence of a liquid metal in the electrolyte. No real connexion of the magnetism with the movements under investigation was detected.

20. To ascertain whether the movements varied with the quantity of the electric current, I prepared a single series of sixty-six pairs of Smee's batteries, forty of which were charged with spring-water, and the remainder with a mixture of one measure of sulphuric acid and fifteen measures of water. The movements obtained on applying the current from the whole series to very dilute sulphuric acid containing a globule of mercury, were much more feeble and the amount of electrolysis much less than when the current from the twenty-six strongly-charged pairs alone was applied. On substituting distilled water for the dilute acid, the movements were stronger and the electrolysis greater with the whole series than with the twenty-six pairs. In all cases the movements appear to be dependent upon the quantity of electricity circulating.

21. It is highly probable, from the experiments just described, that the movements are intimately dependent upon electro-chemical action occurring at the surface of the liquid metal, especially as the amount of motion varies with the quantity of electricity which passes from the electrolyte into the metal, or *vice versa*; and it would be very desirable to obtain a negative proof of this by an experiment with a globule of one liquid metal in a bath of some other liquid metal, as already attempted (12).

22. With every liquid yet examined the movement of the liquid has invariably been attended by a simultaneous movement of the fluid metal; and the greater the movement of the liquid the greater was the movement of the metal, from which I infer that the movements of the two substances are mutually dependent.

23. The results in general indicate that the *sudden* movements are of the same general character as the *continuous* ones, the effect in the former case being heightened by the concentration of the electric force within a small compass, together with the additional electric energy always displayed at the moment of making contact with a battery.

24. The movements require for their production two substances (6); both these substances must be in a liquid state (7), and be conductors of electricity (8); one of them must be a metal or a metallic alloy (9); any metal or alloy will do (10), and only a mere film of it is essential (11); the other must be an electrolyte, and

need not contain water (13); any electrolyte will do (14), and only a thin layer of it is requisite (15); the electric current must pass from the electrolyte into the metal, or *vice versa* (16), but need not pass both into and out of it by the electrolyte (17); the electrodes are not essential (18); the movements are not electro-magnetic (19), they are dependent upon the quantity of the electric current (20), and are intimately connected with electro-chemical action (21); the movements of the metal and electrolyte are mutually dependent (22), and the momentary movements are of the same nature as the continuous ones (23).

25. The pure or abstract conditions of the production of the phenomena are,—a liquid metal (or alloy) in contact with a liquid electrolyte, and a quantity current of electricity passing between them.

B. Conditions of the continuance of the Movements.

26. With regard to the *continuance* of the movements:—1st. In some cases the metal becomes covered with an insoluble film, produced by ordinary chemical action of the liquid, which prevents the continuance of the action; this occurs particularly with mercury in strong solutions of sulphides, iodides, bromides, and chlorides. 2nd. If the positive wire is connected with the mercury and the negative wire with the liquid previous to placing both the wires in the electrolyte, films are in nearly all cases instantly produced (but not with strong sulphuric acid) and interfere with further action; films are also frequently produced by a similar cause upon the end of the mercury nearest to the negative wire when *both* the wires are in the solution, and in many such cases the mercury creeps in a peculiar serpent-like form beneath the film towards the negative wire. 3rd. In many instances the metallic globule becomes of a pasty consistence by absorbing substances deposited upon its surface by electrolysis, and the motion declines; this takes place particularly with mercury in solutions of salts of ammonia, baryta, strontia, magnesia, and lime, but most with those of magnesia and lime; and it occurs very rapidly if, instead of placing both the polar wires in the electrolyte, the negative wire is immersed in the globule of mercury. It is evident from these facts, that it is essential to the continuance of the movements, that the particles composing the surface of the metallic globule should retain a sufficient degree of mobility to admit of free motion.

27. The best method of obtaining a *continuous* movement is to place a globule of pure mercury in a watch-glass, barely cover it with dilute sulphuric (or nitric) acid, connect it with the negative platinum wire and the liquid with the positive platinum wire of a battery of sufficient power to produce a moderate flow without overheating the liquid: ten small Smee's batteries are sufficient. By this means I have obtained undiminished motion for upwards of six hours.

C. Conditions of the direction of the Movements.

28. In speaking of the *direction* of the movements, I always mean those of the supernatant liquid, unless otherwise stated, because the true movements of the mercury are generally less easily detected

than those of the electrolyte : the movements of the liquid are best observed by means of charcoal or asphaltum (5), and those of the mercury by the aid of a few parallel scratches upon the under surface of the watch-glass.

29. The directions of flow of the metal and liquid are intimately dependent upon each other, for in every instance the metal moves in an opposite direction to the electrolyte (see also 22) ; and in those cases where two opposite flows of the liquid towards the centre of the vessel occur (as with a solution of sulphate of potash), the globule of mercury is elongated at both ends into a pointed shape, and its two ends point toward the two electrodes, its largest diameter being directly under the point of meeting of the flows of the solution, and its acutest apex under the strongest flow. The relation between the metal and liquid is apparently of a dual or polar character, the movements of the two bodies being always opposite and equal. This mutual dependence of the motions explains the necessity of *both* the substances being in a liquid state (7).

30. With regard to the *direction* of the flows, there are *three* cases to be distinguished :—1st. The movements obtained by immersing the negative wire in the metal and the positive one in the electrolyte. 2nd. Those obtained whilst the positive wire is in the globule and the negative one in the electrolyte. 3rd. Those produced by immersing *both* the wires in the supernatant liquid with the globule between them.

31. Upwards of 150 different liquids, including organic and inorganic acids, alkalies, salts of alkalies, earths and heavy metals, also organic salts, were examined by the first of these methods, and in almost every instance the flow of the supernatant liquid was *positive*, the clearest exception being with a solution of persulphate of iron. With concentrated sulphuric acid the motion (if any) was very feeble, but with diluted acid of various degrees of dilution it was strongly positive. The flow of the liquid declined quickly with solutions which contained an alkali-metal, apparently in consequence of the mercury becoming less mobile (?) by absorption of that metal ; but with dilute acids it continued a long time ; with very dilute nitric acid, in one experiment the movement was sustained with scarcely any diminution during $2\frac{1}{2}$ hours ; and with dilute sulphuric acid, in a second experiment it continued $6\frac{1}{4}$ hours, and did not then appear to slacken : the battery employed in this experiment consisted of ten small Smee's elements. A globule of strong sulphuric acid was placed upon a surface of mercury, the latter connected with the negative pole, and the end of the positive wire dipped into the acid ; much gas was evolved from the anode, and the liquid was not repelled on making contact, but collected in a heap around the wire ;—a globule of solution of caustic potash similarly treated exhibited repulsion on making contact. It is evident from these uniform results that the direction of flow obtained by immersing the positive wire in the electrolyte and the negative one in the mercury is almost uniformly *positive*.

32. The movements obtained by this method are not produced by

the act of deposited substances dissolving in the mercury, for they occur equally well whether hydrogen gas is set free and escapes or alkali-metal is deposited and dissolves in the mercury, until in the latter case the diminished mobility of the globule interferes with the result.

33. Considerable difficulty was experienced in examining liquids by the second method, in consequence of the rapid and in many cases instantaneous oxidation or filming of the metallic globule; but by using very dilute liquids and immersing the negative wire from seventy-two small Smee's elements during only a moment at a time, this difficulty was in most cases sufficiently overcome to allow distinct starts of the mercury to occur in the particular direction beneath its film, and thus to indicate an opposite motion of the supernatant liquid, although in nearly all cases the movement of the electrolyte itself could not be detected. Upwards of 100 liquids, consisting of organic and inorganic compounds—acid, alkaline, and neutral—were examined, and in more than three-fourths of them distinct movements of the metal were obtained, which were in every instance in a positive direction, thus indicating a *negative* flow of the electrolyte. In some liquids, viz. oil of vitriol, moderately dilute nitric acid, strong solutions of sulphate of ammonia, iodide of ammonium, and sulphite of potash, very dilute solutions of bisulphate of potash, iodide of potassium, nitrate of cobalt, hydrocyanic acid, cyanide of potassium, and acetic acid,—visible movements of the liquid itself in a *negative* direction were also obtained. The movements of the liquid and of the metal very quickly ceased. These experiments show that the direction of flow obtained by placing the positive wire in the metal and the negative wire in the electrolyte is always *negative*.

34. The movements obtained both by methods 1 and 2 appear to be produced by a mutual attraction of the liquid and metal; in the former case the mercury attracts an electro-positive element of the liquid (hydrogen or an alkali-metal), and produces a positive flow; and in the latter case it attracts an electro-negative element (generally oxygen), and produces a negative flow.

35. Herschel found by the third method of operating, that with pure mercury in acids and saline liquids the flow was negative, and was weaker as the base was stronger, and more rapid as the acid was stronger and more concentrated; and that in solutions of nitrates two opposite flows occurred, one from each wire (*vide* Gmelin's Handbook, i. 490). I have found by an examination of pure mercury in various liquids the results exhibited in the following Table. The arrows indicate the direction of flow of the liquid, + being positive and - negative; and the numbers affixed to them afford a rough approximation of the velocity or magnitude of the movements. The battery employed consisted of twenty-two small Smee's elements. The substances were dissolved in water, and the solutions were of moderate strength unless otherwise stated. Manifestly impure substances were rejected, and fresh mercury was taken for each experiment. The results obtained were in many cases verified several times:—

Distilled Water.....	+	faint	Caustic Soda	+	4
Boracic Acid.....	+	8	Carbonate of Soda	+	1
Phosphoric Acid	+	5	Bicarbonate of Soda.....	+	2
Strong Sulphuric Acid...	+	10	Biborate of Soda	+	
Strong Sulphuric Acid 1 measure	+	5	Diphosphate of Soda ...	+	2½
Water 5 „			Sulphide of Sodium, dilute.....	+	2
Strong Sulphuric Acid 1 measure	+	5	Hyposulphite of Soda ...	+	4
Water 15 „			Sulphite of Soda	+	5
Strong Hydriodic Acid 1 measure	+	6	Sulphate of Soda → ⁴ and then → ⁴	+	3
Water 15 „			Chloride of Sodium	+	4
Hydrobromic Acid, very dilute	+	2	Nitrate of Soda.....	+	3
Strong Hydrochloric Acid 1 measure	+	4	Phosphate of Soda and Ammonia	+	4
Water 5 „			Baryta Water	+	2
Strong Hydrochloric Acid 1 measure	+	3	Carbonate of Baryta ...	+	2
Water 15 „			Chloride of Barium.....	+	2
Perchloric Acid, very dilute	+	4	Nitrate of Baryta	+	3
Strong Hydrofluoric Acid 1 measure	+	2	Strontia Water.....	+	2
Strong Hydrofluoric Acid 1 measure	+	3	Chloride of Strontium ...	+	2
Water 5 „			Nitrate of Strontia	+	
Strong Nitric Acid, 1 measure	+	2	Sulphate of Magnesia ...	+	3
Water 1 „			Chloride of Magnesium, strong	+	
Strong Nitric Acid, 1 measure	+	3	Chloride of Magnesium, weak	+	
Water 5 „			Nitrate of Magnesia, strong	+	
Strong Nitric Acid, 1 measure	+	3	Nitrate of Magnesia, weak	+	2
Water 15 „			Lime Water	+	1
Aqueous Ammonia, strong	+	2	Sulphate of Lime	+	2
Sesquicarbonate of Ammonia	+	4	Chloride of Calcium ...	+	2
Phosphate of Ammonia ..	+	2	Chloride of Calcium in Alcohol	+	1
Sulphide of Ammonium, 1 measure	+	2	Nitrate of Lime	+	4
Water 15 „			Sulphate of Alumina ...	+	3
Sulphate of Ammonia ...	+	3	Potash Alum.....	+	4
Hydrochlorate of Ammonia	+	3	Hydrofluosilicic Acid!...	+	4
Nitrate of Ammonia.....	+	3	Silicate of Potash.....	+	4
Caustic Potash	+	2	Molybdate of Ammonia ..	+	
Carbonate of Potash	+	1	Chloride of Chromium, very weak	+	1
Bicarbonate of Potash ...	+	4	Monochromate of Potash ..	+	2
Sulphide of Potassium, dilute	+	1	Nitrate of Uranium	+	2
Sulphite of Potash	+	2	Sulphate of Manganese ..	+	
Sulphate of Potash	+	3	Arsenic Acid.....	+	2
Bisulphate of Potash ...	+	3	Arseniate of Ammonia...	+	3
Iodide of Potassium ..	+	3	Fluoride of Antimony...	+	2
Bromide of Potassium...	+	2	Antimoniate of Potash...	+	4
Chloride of Potassium ...	+	4	Nitrate of Bismuth	+	2
Chlorate of Potash	+	3	Sulphate of Zinc	+	4
Nitrate of Potash	+	3	Iodide of Zinc, strong ...	+	
			Nitrate of Zinc	+	3
			Iodide of Cadmium	+	2
			Iodide of Tin, strong ...	+	6
			Nitrate of Lead	+	3
			Protosulphate of Iron ...	+	4
			Persulphate of Iron.....	+	
			Chloride of Cobalt, weak ..	+	
			Nitrate of Cobalt	+	3

Sulphate of Nickel	+4	Oxalate of Ammonia ...	+3	+4
Nitrate of Nickel	+5	Acid Oxalate of Potash...	+3	
Sulphate of Copper, weak	+	Neutral Oxalate of Potash	+4	+3
Chloride of Copper, very		Formic Acid.....	+3	
weak	+	Acetic Acid	+3	
Nitrate of Copper.....	+3	Acetate of Potash.....	+3	+1
Nitrate of Mercury	+1	Acetate of Soda	+3	+3
Strong Aqueous Hydro-		Acetate of Baryta.....	+2	+2
cyanic Acid	+2	Acetate of Uranium.....	+faint	
Strong Aqueous Hy-		Acetate of Zinc.....	+4	
drocyanic Acid,		Acetate of Lead	+1	
4 measures	+10	Acetate of Copper	+1	
Aqueous Ammonia,		Tartaric Acid	+5	+1
1 measure		Monotartrate of Potash.	+3	+2
Cyanide of Potassium...	+10	Bitartrate of Potash.....	+5	
Strong Aqueous Hy-		Bitartrate of Soda	+3	+½
drocyanic Acid,		Tartrate of Potash and		
5 measures	+8	Soda	+3	+3
Caustic Soda Solution,		Tartrate of Potash and		
1 measure		Antimony	+3	
Cyanide of Mercury.....	+faint	Citric Acid	+4	
Ferrocyanide of Po-		Succinic Acid	+4	
tassium	+4	Gallie Acid	+2	
Sulphocyanide of Po-		Pyrogallie Acid	+½	
tassium	+1	Carbazotic Acid	+4	
Oxalic Acid	+2	Benzoic Acid.....	+2	

Numerous interesting phenomena of motion and of colour, especially with solutions of salts of the earth-metals and with metallic iodides, were observed during the examination.

36. On examining these numerous results we find :—1st. That all alkalies and some alkaline salts produce a positive flow only. 2nd. That some alkaline and many neutral salts produce both positive and negative flows. 3rd. That some neutral and many acid salts, and nearly all acids, both organic and inorganic, produce a negative flow only. The stronger influence of acids, compared to that of alkalies (14, Note) in the production of these movements, is probably the reason why various salts of alkaline reaction give a negative as well as a positive flow, and why many neutral salts containing a strong acid (chlorides, for example) give a negative flow only. No substance of alkaline reaction has been observed to give a negative flow only, nor any strongly acid substance to give only a positive flow. An *alkaline* or electro-positive substance as the electrolyte, produces therefore by the 3rd method a *positive* flow, and an acid or electro-negative substance produces a *negative* flow. Numerous analogies may be detected in the behaviour of similar salts on examining the Table.

37. The movements obtained by the 3rd method appear to be results of a similar mutual attraction of the mercury and the elements of the liquid to those obtained by methods 1 and 2. The mercury moves towards the cathode in acids because its positive end has acquired, by the aid of the electric current, a stronger affinity for the negative element of the liquid than its negative end has for the positive element, and moves towards the anode in alkalies because its negative end has acquired a stronger attraction for the positive

element of the liquid than its positive end has for the negative element. *I do not, however, give either this or the previous explanation (34) as an ascertained fact, but merely as a temporary hypothesis to aid further investigation.*

38. The amount of positive flow produced by the 3rd method in strong aqueous hydrocyanic acid, or strongest solution of ammonia, is comparatively small, apparently on account of their inferior electric conductivity; but if the smallest amount of ammonia is added to the hydrocyanic acid, the positive flow obtained is very strong; also, if instead of ammonia a small quantity of caustic potash, soda, baryta, strontia, magnesia, lime, or even alumina is added to the acid, similar effects are produced: a little strontia or lime causes the nearest part of the mercury to dart up the watch-glass more than half an inch towards the positive electrode, if the battery is sufficiently strong. Silica had no effect. The addition of oxide of zinc, dioxide or protoxide of copper to the acid, reversed the direction of the flow, and dioxide of mercury neutralized the positive movement and diminished the conduction. The strongest positive flows obtained by the 3rd method were with strong solutions of alkaline cyanides, and the strongest negative flows with sulphuric acid.

39. The behaviour of liquids upon mercury in V-tubes by the three methods is not essentially different from their behaviour in a watch-glass; the former, indeed, may be safely predicted from the latter. Sufficient pure mercury was placed in a V-tube of half an inch bore just to fill it at the bend, then a strong solution of sulphate of alumina poured upon it half an inch deep in each leg; on connecting the platinum wires from twenty-two pairs of small Smee's batteries with the solutions in the two legs, the liquid at once flowed from the *negative to the positive* leg; but by lowering the negative wire into the mercury, it flowed from the positive to the negative leg: no flow of the liquid was produced by placing the positive wire in the mercury and the negative one in the solution of the negative leg. If the mercury was too deep to allow the liquid to pass, the solution insinuated itself down the sides of the mercury in the positive leg (the positive wire being in the solution, and the negative one in the mercury); but by using a suitable depth of mercury, the whole of the liquid flowed from the positive into the negative leg. This is the usual behaviour of an *acid* liquid (or of one in which the negative flow of method 3 predominated) with a suitable quantity of mercury in a V-tube. With a *strongly alkaline* liquid the only difference of behaviour is, that when the two wires are in the solutions of the two legs, the liquid flows from the *positive to the negative* limb (see 16), *i. e.* opposite to the direction of flow with an acid.

40. There is a fixed relation between the direction of the electric current and the direction of each of the classes of movements; for in every case where the former is reversed, the latter also becomes reversed; but this effect is, of course, not observable in those cases of method 3 where two opposite and equal motions to the centre of the metallic globule exist.

41. I have examined the influence of the chemical nature of the

metallic globule upon the movements obtained by the 1st method, in the following manner. The globule of mercury was first connected with the positive pole and the liquid with the negative pole for about ten seconds, and then the wires placed as in method 1; a *temporary negative* flow was produced for a few moments with certain liquids, apparently in consequence of the mercury absorbing a minute portion of an electro-negative constituent of the solution (?), and that substance causing a negative flow in the succeeding operation until the whole of it was redissolved. The following liquids exhibited this phenomenon of reversion:—very dilute solutions of nitric acid, nitrates of ammonia, potash, soda, baryta, strontia (not of magnesia, apparently on account of viscosity of the mercury being produced), lime, zinc, lead, cobalt, nickel, copper, and dioxide of mercury; also sulphates of ammonia and potash; hypophosphite and diphosphate of soda; and, strongest, the alkaline nitrates;—but not dilute solutions of caustic potash, soda, baryta, or lime; carbonates or bicarbonates of potash or soda; carbonate of baryta; chlorides of ammonium, potassium, sodium, barium, strontium, magnesium, or calcium; iodide or bromide of potassium; sulphites of potash or soda; biborate, hyposulphite, or sulphate of soda; sulphate of lime; arsenic acid; cyanide of potassium; oxalate of ammonia. The battery used was a series of 72 small Smee's elements. It appears from these experiments, that the direction of flow obtained by immersing the positive wire in the electrolyte and the negative one in the globule is strongly influenced by the chemical composition of the metallic globule.

42. The chemical nature of the globule exercises an equally powerful influence upon the direction of the movements obtained by the second method. If the mercury was first connected with the negative wire and the solution with the positive wire for a few seconds, and then the connexions reversed or made as in method 2, a *temporary* and strong *positive* flow of the electrolyte for a few moments was obtained, apparently in consequence of the mercury absorbing a little alkali-metal or other electro-positive constituent of the liquid, and that substance causing a positive flow of the solution until the whole of it was redissolved. This positive flow did not occur while there was above a certain quantity of the alkali-metal in the mercury. The reversions were obtained in the following liquids:—dilute and strong solutions of caustic potash; weak solutions of caustic soda, baryta, and lime; carbonate of baryta; chlorides of potassium, sodium, barium, strontium (not of magnesium, owing to viscosity of the globule), and calcium; iodide and bromide of potassium; sulphites of potash and soda; biborate, hyposulphite, and sulphate of soda; sulphate of lime; arsenic acid; cyanide of potassium; and oxalate of ammonia; also in solutions of hypophosphite and diphosphate of soda;—but not in very dilute nitric acid, nitrates of ammonia, potash, soda, baryta, strontia, magnesia, lime, uranium, zinc, cobalt, nickel, copper, or dioxide of mercury; sulphates of ammonia, potash, or alumina. It is worthy of notice that these two series are almost precisely the reverse of those named with method 1 (41); *i. e.* those liquids which have the property of reversing the flow of

one method have not that property with the other method, except hypophosphite and diphosphate of soda. The explanation suggested (34), of the cause of the true movements of methods 1 and 2 does not appear applicable to these phenomena of reversion.

43. Herschel has shown that with pure mercury in solutions of alkalies or of sulphate of soda (*vide* Gmelin's 'Handbook,' i. 490, 492), if a little alkali-metal be introduced into the globule by connecting the latter for a few moments with the negative wire (the other wire being in the solution), a *positive* flow occurs on placing *both* the wires in the electrolyte with the mercury between them, and continues until all the alkali-metal is redissolved; and that similar effects are produced by adding small quantities of an easily oxidizable metal to the mercury—for example, potassium, sodium, barium, zinc, iron, tin, lead, or antimony, in the order given; but not by bismuth, copper, silver, or gold. I have found that zinc added to mercury under a solution of sulphate of potash changed the direction of flow from positive and negative (obtained by method 3) to positive only; cadmium did the same, but more feebly, and tin still more feebly; bismuth had no apparent effect, but by using treble the electric power its effect was also similar, antimony also the same; gold had no apparent effect even with a current from 72 pairs of Smee's elements. No positive flow was obtained by connecting the mercury with the negative wire and the solution with the positive wire for a short time in a liquid consisting of acid and water, and then placing both the wires in the electrolyte. Although there are many liquids (most of those which contain an alkali-metal) in which a temporary *positive* flow (or *increase* of positive flow) may be obtained by the 3rd method by first placing the *negative* wire in the mercury for a short time and then returning it to the electrolyte, there are but few (among which are diphosphate of soda and arseniate of ammonia) in which a temporary *negative* flow is produced by placing the *positive* wire in the mercury and then returning it to the solution. It has been constantly observed with the 3rd method, that purity of the mercury is essential to the production of uniform results. From these various facts it appears that the chemical nature of the metallic globule strongly influences the direction of the movements obtained by method 3; also that an electro-positive globule produces a positive flow, and an electro-negative substance dissolved in the mercury produces a negative flow.

44. In some instances of the 3rd method—for example, with solutions of chloride of magnesium and nitrate of magnesia (35, Table), even the degree of dilution appears to determine the direction of the motion. No variation in the direction of the movement obtained by either method was observed on varying the strength of the electric current, or on varying either the actual or relative distances of the electrodes from the metallic globule.

45. The presence of an electro-positive metal in one portion of the surface of the mercury will (by generating a small electric current) sometimes cause rotation of the electrolyte after the battery-wires are removed, especially if the mercury is touched with a platinum wire beneath the surface of the liquid; this is seen most

frequently with mercury into which some alkali-metal has been deposited.

46. The general phenomena of the movements may be briefly redescribed thus:—A. When *both* the wires are in the electrolyte, and the mercury between them, several cases occur: 1. With a strongly *alkaline* liquid, a *positive* flow of the solution from the positive wire over the mercury to the negative wire occurs; 2. With a strongly *acid* liquid, a *negative* flow of the solution takes place; and 3. With a solution of a *neutral* or slightly alkaline salt, especially of a salt composed of a strong acid and a strong base, *two* flows occur, a negative one from the negative wire towards the centre of the mercury, and a positive one from the positive wire towards the centre of the mercury,—the negative one being generally the strongest. If in this 3rd case the mercury contains any impurity, or if a substance be caused by any means to dissolve in the mercury, the movements are notably affected: an electro-positive substance (zinc, alkali-metal, &c.) increases the positive flow so as partly or completely to overpower the negative movement; and an electro-negative substance increases the negative flow, in a few instances, so as to overpower the positive movement. These influences are also frequently detectable when liquids are used of alkaline or acid reaction, as in cases 1 and 2.

B. When the negative wire is in the mercury and the positive one in the liquid, two cases occur: 1. With pure mercury, the motion is positive in nearly all liquids, whether acid, alkaline, or neutral; and 2. With mercury containing a small amount of an electro-negative substance, imparted to it by reversing the connexions of the wires for a short time, a temporary negative flow is produced in certain liquids, chiefly nitrates, but not in certain other liquids.

C. When the positive wire is in the mercury and the negative one in the liquid, also two cases occur: 1. With pure mercury, the motion is negative in all liquids—acid, alkaline, or neutral; and, 2. With mercury containing a *small* quantity of an electro-positive substance imparted to it by reversing the connexions of the wires for a few moments, a temporary and strong positive flow is produced in certain liquids and not in certain others—and these liquids are almost precisely the reverse of those named under B, 2.

The general influence of electro-positive substances dissolved in the *globule* is in all classes of cases to produce a positive flow, and of electro-negative substances to produce a negative motion; and the influence of electro-positive substances dissolved in the *liquid* is, in cases of A only, to produce a positive flow, and of electro-negative substances to produce a negative flow.

47. The primary motions of the liquid and metal are, in all cases, wholly at their *surfaces of mutual contact*; whilst the movements observed are only secondary effects, useful in enabling us to infer the direction of the original motions: the *masses* of liquid and metal serve merely as conductors of the electricity, and as stores of material for supplying the acting surfaces. The movements obtained are singularly symmetrical, probably in consequence of their essentially dual or polar character.

48. The essential nature or principle of the movements appears to be *electro-chemical motion*, i. e. definite motion directly produced by electro-chemical action.

49. To illustrate the action, I have constructed an apparatus consisting of two pairs of electrodes of platinum foil and mercury, suspended at opposite ends of two copper wires upon a central pivot, and rotating in an annular channel filled with dilute sulphuric acid; but the power was too feeble to produce revolution of the necessary moveable parts: it was not more than sufficient to produce a manifest tendency to motion.

In conclusion, I beg leave to suggest a trial of the sudden starts of the mercury by momentary currents as signals in electro-telegraphic apparatus.

GEOLOGICAL SOCIETY.

[Continued from p. 86.]

May 16, 1860.—L. Horner, Esq., President, in the Chair.

The following communication was read :—

“ Outline of the Geology of part of Venezuela and of Trinidad.”

By G. P. Wall, Esq.

The district examined by Mr. Wall extends from the 8th degree north latitude to the sea, and eastward of the 69th meridian. It includes the Serranía (as the mountainous region is termed) and the Llanos or plains to the south. The Serranía is a portion of the Littoral Cordillera, and is continuous with the main ridge of the Andes; to the east it extends into the northern part of Trinidad.

The most ancient rocks in Venezuela consist of mica-schists and gneiss, and compose the author's “Caribbean Group,” so called on account of these rocks forming for a great distance the southern boundary of the Caribbean Sea. This term was previously adopted for the same series of mountain-rocks in Trinidad, where their eastward extension had been already studied. These strata consist of—1st, a series of micaceous and siliceous schists, various in aspect and constitution; 2ndly, sandstones, coarse and micaceous, fine-grained and without mica; 3rdly, shales, ferruginous, micaceous, and carbonaceous.

These schistose rocks are highly distorted. In the western portion of the district they have a breadth of about 30 miles, and rise to the height of 8000 feet; to the east they form lower ridges and a narrower belt. They are interlaminated with numerous irregular bands of quartz. Evidence is not wanting of the segregation of the quartz having been subsequent to the formation of the strata, but previous to the disturbances that the schists have undergone.

White and blue limestones, generally crystalline, but sometimes compact, occur in the schists in Venezuela, but still more abundantly in Northern Trinidad. Some of the schists are granatiferous; and, from the presence of smaragdite, they sometimes form an eklogite.

Gneiss also is present, and is markedly interstratified with the mica-schists. The transition is occasionally gradual; but more usually it is sudden and abrupt. The gneiss sometimes assumes the irregular structure of granite, but is still distinctly bedded. It

is occasionally auriferous. Very small proportions of copper-ores and argentiferous galena exist in some localities.

The Serranía also comprises another great group of strata, flanking the "Caribbean" rocks on the south, and in the eastern district rising to a height of more than 7000 feet, with a breadth of from 30 to 40 miles. These consist of sandstones, fossiliferous limestones, and shales; and form the group provisionally termed "Older Parian" by the author, from the circumstance of its occurring on the shores of the Gulf of Paria. In Trinidad it is traced as a narrow belt across the island.

Alternating with the sandstones and limestones are thick beds of a rock containing 85 per cent. of clay and only about 3 per cent. of carbonate of lime. This is provisionally termed "Argilline." Another peculiar bedded rock of this series is composed of nearly pure and very fine siliceous matter: it is noticed by the author under the name of "Chertine."

These Older Parian strata must be nearly 8000 feet thick. They have been intensely disturbed. Though the fossils can rarely be separated from the matrix, yet some were fortunately obtained from near Cumaná,—namely, *Trigonia* and small *Gasteropoda*. Mr. Etheridge, F.G.S., suggests that most probably they indicate a Lower Cretaceous age. These Lower Parian rocks extend westward into New Granada; and are probably related to the Neocomian rocks of Bogotá.

Near their junction with the "Caribbean Group," the Older Parian strata are often interstratified and alternate with rocks of igneous origin. The base of these pyrogenic rocks varies from augitic to diabasic in their type.

The Llanos or grassy plains of Venezuela are entirely formed of conglomerates and sandstones referable to the next great group of strata, namely the "Newer Parian." In Trinidad a lower and calcareous portion of this group exists. Altogether this group probably has a thickness of nearly 4000 feet. In the plains of the mainland the strata are horizontal; but in Trinidad they have suffered great disturbance.

Fossils are abundant in the calcareous division, and seem to represent the Lower Pliocene or Upper Miocene series of Europe.

The materials of the conglomerates and sandstones have been mainly derived from the disintegration of the Lower Parian rocks.

The upper portion of the Newer Parian series, which is often shaly, contains beds of lignite, frequently admitting of exploitation. The lignite occurs at several localities on the mainland, and also in Southern Trinidad. The lignitiferous beds have locally undergone combustion to a great extent, from natural causes, such as the decomposition of pyrites. The result is that the strata have been indurated and baked for a vertical extent sometimes of 70 or 80 feet; the clays assuming various conditions, and presenting the "porcelanites" and "thermantides" of continental authors.

The asphalt of Trinidad and the mainland is almost invariably disseminated in the upper part of the Newer Parian group. When *in situ*, it is confined to particular strata, which were originally shales

containing a certain proportion of vegetable *débris*. The organic matter has undergone a special mineralization, producing a bituminous, in place of the ordinary anthraciferous substances. This operation is not attributable to heat, nor of the nature of distillation; but is due to chemical reaction at the ordinary temperature and under the normal conditions of the climate. After the solution and removal of the bitumen from wood passing into asphalt, the remaining organic structure presents peculiar appearances under the microscope.

The occurrence of asphalt in New Granada and the Valley of the Magdalena in all probability indicates the presence of the Newer Parian strata in those districts.

The phenomena of salses or mud-volcanos (common in Trinidad and on the mainland) are referred by the author to the chemical decomposition and changes (such as the formation of asphalt) which are in operation in the lignitic strata of this formation, since the evolved gases are inflammable, and the discharges of muddy matter are usually accompanied with asphalt and petroleum.

Thermal waters are not rare. Those of Trincherras, issuing from mica-schist, possess a variable temperature, according to the following determinations:—Humboldt in 1800, 194° ; Boussingault in 1823, 206° ; and the author in 1859, 198° . The hot springs at Chaguaranal near Pilar are much more interesting, as the water is discharged, from a limestone of the Older Parian series, at a temperature even above the boiling-point. It deposits carbonate of lime, sulphur, &c.

A souffrière near by, issuing from an Older Parian sandstone, yields a variety of purely siliceous deposits, chalcedonic and agatiform. The siliceous cement of the sandstone, being a hydrate, and soluble in acidulated water, has probably afforded the material for these deposits. The colour of these siliceous sinters are traceable, some to the sulphur (yellow), others to the decomposing leaves (brown, &c.).

Earthquakes are of ordinary occurrence. The earthquake of 1853 destroyed the town of Cumaná.

The probable relations of the Cretaceous rocks of Venezuela with those of New Granada, Peru, Chili, Brazils, and the Straits of Magellan were alluded to by the author.

XIX. *Intelligence and Miscellaneous Articles.*

ON THE FORMATION OF ICE AT THE BOTTOM OF WATER.

BY M. ENGELHARDT.

THE author in 1829, undertook some experiments with a view to explain the formation of ground-ice and has since repeated them.

He took three iron boilers about 660 millims. in diameter, and a wooden trough about the same dimensions. These were severally filled with river-water of the temperature $+2^{\circ}$: the temperature of the air was -2° during the day, and sank to -5° during the night. They were supported at a distance of 20 centims. from the ground, so

as to have the same temperature on all sides. The next morning all the vessels were covered with a layer of ice of about 12 to 14 millims. thick; and at the bottom and sides of the three iron vessels there was a smooth layer of ice about 20 millims. thick, that on the bottom being thinner. In the wooden vessel there was a layer about 2 millims. thick, and a few tufts of needles on the sides; and on the bottom there were a few isolated plates of ice 100 millims. long, 5 to 7 millims. wide, and 1 to 2 millims. thick, with other small plates on the edges like the teeth of a saw. These experiments were repeated several times, and always gave the same result; that is, the vessels were covered on the sides and bottom with a layer of ice, the thickness of which varied with the conductibility and radiating power of the vessel.

To observe the formation of ice at the bottom of water, iron dishes of about 5 centims. depth were filled with water and placed on a freezing mixture of ice and salt. The temperature of the room was $+15^{\circ}$; and consequently there was no formation of ice on the surface, but on the bottom. The congelation did not always take place in the same manner. Sometimes needles formed at the bottom, and gradually increased until the ascending force caused by their lower specific gravity forced them to break loose and rise to the surface. At other times the bottom was rapidly covered with a thin layer of ice, sometimes marked with fine lines.

To come to the formation of ice at the bottom of rivers. The water at a temperature of 0° only loses heat on the surface by radiation, or by contact with colder bodies. The earth at the bottom and sides of rivers is a bad conductor, but water and ice are still worse conductors. Ice being specifically lighter than water, always swims to the surface when its ascending force is able to overcome its adhesion to the bottom. It can also carry with it heavy bodies from the bottom of the water.

As the maximum density of water is at 4° , all large and tranquil masses of water, and even those which have such a motion that the layers are not prevented arranging themselves according to their specific gravity, are at a temperature above 0° at the bottom of the water, even when they are frozen on the surface. A pond at Niederbronn, which is only a metre deep, was covered with ice at the surface, while the water in the inside was $+3^{\circ}$, the temperature of the air being -11° .

Large masses of water never freeze at the bottom; and even when ice does form, it becomes detached and rises to the surface. But when water at 0° is in a vessel also at 0° , ice forms at the bottom as well as at the surface. In order that ice may form at the bottom of water, it is necessary that the lower layers be cooled to 0° , and even a little more, that this cold water sink to the bottom of the river, cool the sides, and finally find in the midst of the motion something at rest where it can exert its force of adhesion and crystallization.

An obstacle placed in the current of water produces two different effects. It either changes the direction of the liquid molecules which strike against it, and gives them rotatory movements strong

enough to form eddies, or the liquid behind the obstacle comes to rest, and these are stationary and almost immoveable points.

These are good conditions for the formation of ice at the bottom of rivers. The eddying motion produced by obstacles brings cold water to the bottom and cools the sides; and the molecules of water behind the obstacle exert their adhesive force and crystallize. But to produce these effects an intense and continuous cold is necessary.

In conclusion, the author attributes the formation of ground-ice to obstacles in the current, which on the one hand, by the eddying motion, cause the water below 0° to sink to the bottom and cool the sides, and on the other hand produce stationary parts in which the crystallizing power can exert its force. He observed the influence of these foreign bodies in a conduit at Zinsweiler. In 1829, ice formed at the bottom of the water in which there were large stones, trees, &c. The formation of ice was entirely prevented by removing these foreign bodies.—*Comptes Rendus*, July 2, 1860.

ON A REMARKABLE ICE SHOWER. BY CAPTAIN BLAKISTON, R.A.

[Extract of a Letter to General Sabine, R.A.]

"On the 14th January, 1860, when two days out from the Cape of Good Hope, about three hundred miles S.S.E. of it, in lat. $38^{\circ} 53'$ S., long. $20^{\circ} 45'$ E., we encountered a heavy squall with rain at 10 A.M., lasting one hour, the wind shifting suddenly from east to north (true). During the squall there were three vivid flashes of lightning, one of which was very close to the ship; and at the same time a *shower of ice* fell, which lasted about three minutes. It was not hail, but irregular-shaped pieces of solid ice, of different dimensions, up to the size of half a brick. The squall was so heavy that the topsails were let go.

"There appears to have been no previous indication of this squall; for the barometer at 6 P.M. on the two previous days had been at 30.00, therm. 70° ; at 8 A.M. on the 14th, 29.82, therm. 70° ; at 10 A.M. (time of squall), 29.86, therm. 70° ; and at 1 P.M., when the weather had cleared, wind north (true), 29.76, therm. 69° ; after which it fell slowly and steadily during the remainder of the day and following night*.

"As to the size of the pieces of ice which fell, two, which were weighed after having melted considerably, were $3\frac{1}{2}$ and 5 ounces respectively; while I had one piece given me, a good quarter of an hour after the squall, which would only just go into an ordinary tumbler. And one or two persons depose to having seen pieces the size of a brick.

"On examination of the ship's sails afterwards, they were found to be perforated in numerous places with small holes. A very thick glass cover to one of the compasses was broken.

"Although several persons were struck, and some knocked down on the deck, fortunately no one was seriously injured."—*From the Proceedings of the Royal Society for May 3, 1860.*

* The weather on the morning preceding the squall was clouded, with close and thick atmosphere, wind E. (true), 3. By night of the 14th the wind had hauled to N.W. (true), 4; and the day following was W.S.W. (true), 5—6, cloudy.

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[FOURTH SERIES.]

SEPTEMBER 1860.

XX. *On the Radiative Powers of Bodies with regard to the Dark or Heat-producing Rays of the Spectrum.* By BALFOUR STEWART, M.A.*

1. **I**N their communication which appeared in the August Number of this Magazine, Professors Kirchhoff and Bunsen have furnished us with some very interesting and important facts regarding the spectra of different kinds of flame: the following is one of these:—

“It appears,” they say (while describing the effect produced upon flame-spectra by the presence of small quantities of metals or bodies containing metals), “that the alteration of the bodies with which the metals employed were combined, the variety in the nature of the chemical processes occurring in the several flames, and the wide differences of temperature which these flames exhibit, *produce no effect upon the position of the bright lines in the spectrum which are characteristic of each metal.*”

2. Now when the particles of the body which mingle with and characterize the flame are exceedingly divided, we may with much probability (as far as the *quality* of the radiated light is concerned) regard them as ultimate particles or molecules: the spectra exhibited will then afford us the means of approximately ascertaining what kind of light the molecules of certain substances give out when heated. It thus appears that we have grounds for supposing that the ultimate particles of different substances which possess in common some characterizing metallic element, give out the same *quality* of light when introduced into flame.

3. It becomes interesting to know if we have any means of detecting a similar property of bodies (should such exist) having

* Communicated by the Author.

reference to radiant heat rather than to radiant light—if we can by any method ascertain whether there are bodies the particles of which give out the same *quality* of heat at the same temperature. A law which flows from Prevost's theory of exchanges, as proved by the author in a paper published in the last volume of the Transactions of the Royal Society of Edinburgh, affords, it is believed, a method of ascertaining this fact.

4. This law asserts that when a number of bodies exist in an enclosure at a uniform and constant temperature, the heat absorbed by any particle is equal to that emitted by it; and that this equality subsists with regard to every individual description of heat which goes to form the heterogeneous radiation of that temperature.

5. Let us now suppose two plates M and N, composed of two different substances more or less diathermanous, to be hung up side by side in such an enclosure of which the temperature is $=t$; and for the sake of simplicity we may conceive the walls of the enclosure to be covered with lampblack, a substance which radiates, but which does not reflect heat. Let us also suppose that the refractive indices of the two plates are the same, or nearly so; and finally, let us limit our consideration to those rays which enter each plate at right angles to its surface. It may be shown that the same quantity of heat will flow through the substance of each plate.

6. Before the investigation be proceeded with, one hypothesis requires to be made; but one which, besides its apparent probability, has been proved by the author to hold good for mica and glass, viz. that the mere heating of a substance through a considerable range of temperature, if unaccompanied by any chemical change, does not alter the absorptive power of its particles for a given description of heat.

7. Let $a_1, a_2, a_3, \&c.$ denote the absorptive power of the plate M for the different kinds of heat which compose the lampblack radiation of the temperature t , and let $b_1, b_2, b_3, \&c.$ denote the same constants for the plate N.

(By the absorptive power of a substance is meant the quantity of heat which would be absorbed by a plate of the substance of thickness = unity when traversed by a ray of heat, the intensity of which is kept equal to unity throughout the whole of this thickness.)

Let $A_1, A_2, A_3, \&c.$ be the quantities of the different descriptions of heat which together compose the whole radiation which flows perpendicularly through M and N at the temperature t .

Also let $M_1\delta\tau, M_2\delta\tau, M_3\delta\tau, \&c.$ denote the radiations for these different kinds of heat of a particle or plate of exceedingly small thickness $\delta\tau$ of the body M at the above temperature; and let

$N_1\delta\tau'$, $N_2\delta\tau'$, $N_3\delta\tau'$, &c. denote the same constants for the body N for the thickness $\delta\tau'$. It is here supposed that, for exceedingly small thicknesses of the substances M and N, the radiation (and consequently the absorption) is proportional to the thickness. This will only hold good when such thicknesses absorb only a very small proportion of the incident heat. It is, however, possible that there are some kinds of heat which are greatly absorbed even by a single set of particles. If there be such rays, they may be supposed to be excluded from this investigation, as the test to which it refers, and which is furnished by plates of sensible thickness supposed capable of passing heat, is evidently in their case quite inapplicable.

8. Conceive now the plate M to be composed of a great number of slices laid side by side, the thickness of each slice being $\delta\tau$; also let N be composed of the same number of slices, the thickness of each being $\delta\tau'$. Let us denote the total thickness of M by τ , and that of N by τ' .

The quantity of the heat A_1 which will be absorbed by the first elementary slice of M will be $= A_1a_1\delta\tau$, while, again, the quantity of the same heat which will be radiated by the same slice will be $= M_1\delta\tau$. Hence (art. 4) $M_1\delta\tau = A_1a_1\delta\tau$, and

$$\therefore M_1 = A_1a_1.$$

In like manner,

$$\left. \begin{array}{l} M_2 = A_2a_2, \\ \text{\&c.} \end{array} \right\} (1)$$

Also with regard to N, we have, similarly,

$$\left. \begin{array}{l} N_1 = A_1b_1, \\ N_2 = A_2b_2, \\ \text{\&c.} \end{array} \right\} (2)$$

9. Now if the quality of the heat radiated by a particle of M is the same as that radiated by a particle of N, we have

$$M_1 : M_2 : M_3, \text{\&c.} :: N_1 : N_2 : N_3, \text{\&c.};$$

hence, (1) and (2),

$$A_1a_1 : A_2a_2, \text{\&c.} :: A_1b_1 : A_2b_2, \text{\&c.};$$

hence also

$$a_1 : b_1 :: a_2 : b_2 :: a_3 : b_3, \text{\&c.}$$

Take $a_1\tau = b_1\tau'$, hence

$$\tau : \tau' :: b_1 : a_1 :: b_2 : a_2, \text{\&c.}$$

Hence

$$\tau a_2 = \tau' b_2, \quad \tau a_3 = \tau' b_3, \text{\&c.}$$

Also

$$\left. \begin{aligned} \delta\tau a_1 &= \delta\tau' b_1, \\ \delta\tau a_2 &= \delta\tau' b_2, \\ &\&c. \\ \delta\tau a_m &= \delta\tau' b_m. \end{aligned} \right\} \dots \dots \dots (3)$$

Now the quantity of heat Λ_m absorbed by the first elementary slice of $M = \Lambda_m a_m \delta\tau$, and for N this is $= \Lambda_m b_m \delta\tau'$. Hence from (3) it follows that the quantity of the heat Λ_m absorbed by an elementary slice of M is equal to that absorbed by a similar slice of N . Hence the quantity of heat Λ_m which is absorbed in passing through a thickness of $M = \tau$ is equal to that which is absorbed in passing through a thickness of $N = \tau'$; and the same equality subsists with respect to any other description of heat which forms part of the heterogeneous radiation of the temperature t .

Hence it follows that, with the above relations between the thicknesses of the plates, the portion of lampblack heat of the temperature t which passes the one plate is equal in quality as well as quantity to that which passes the other.

10. It thus appears that if there are bodies of which the ultimate particles or very thin plates radiate the same quality of heat at the temperature t , and if such thicknesses of these bodies be taken that they all pass the same proportion of the incident heat for any one of the rays which compose the heterogeneous radiation of that temperature, then they will also pass the same proportion for any other of these rays.

11. Our hypothesis has hitherto been, that the bodies M and N have ultimate particles which radiate the same kind of heat at the temperature t . Let us further suppose that the particles of these bodies possess the same property at the temperature t' , and in fine through a considerable range of temperatures.

At the temperature t' let us form equations similar to (1), (2), (3). Now if we suppose, as we are undoubtedly entitled to do, that the heat of t' has some one ray in common with that of t , we see at a glance that the plates M and N with their old thicknesses will perform the same office for the heat of temperature t' which they did for that of temperature t , the proportion of the former heat which passes being the same for M as for N in quantity and in quality.

12. To conclude: *if there be a group of bodies of nearly the same refractive index the particles of which always radiate the same quality of heat at the same temperature, and if we take slices of these bodies of thicknesses such that they all permit to pass the same proportion of any one kind of heat, then they will also pass the same proportion of any other kind of heat.*

13. We are thus furnished with a test by means of which we may in all probability ascertain whether such groups of bodies exist. In applying this test, it will be necessary to assume the truth of the hypothesis of art. 6. Our method will be to construct screens of the bodies under analysis such that they all stop the same proportion of some one kind of incident heat; and if these bodies possess the property we are in search of, that is, if their ultimate particles always radiate the same quality of heat at the same temperature, the screens will all stop the same proportion of any other description of heat. It is almost unnecessary to remark that the common proportion stopped by the screens for one kind of heat will generally be different from that stopped by them for heat of another description.

14. This test has possibly a still wider application. For let it be supposed that we are comparing together two bodies which possess in common the same characterizing or influential element, but one of which possesses in addition another element also influential, but relating to an entirely different set of rays. It seems likely that for those kinds of heat which are characteristic of the common element the bodies will conform to the test; but, on the other hand, for those kinds of heat which refer to the element which the one body possesses and not the other, the test will fail.

Some tables furnished by Melloni appear to confirm this idea; but the subject will doubtless require a special experimental investigation.

Kew Observatory,
August 7, 1860.

XXI. *Note on Professors Kirchhoff and Bunsen's Paper "On Chemical Analysis by Spectrum-observations."* By Professor SWAN.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ON perusing Professors Kirchhoff and Bunsen's paper "On Chemical Analysis by Spectrum-observations," I find it stated that I have "already remarked upon the small quantity of sodium necessary to produce the yellow line in a flame-spectrum." As I have not only remarked that a small quantity of sodium suffices to produce the yellow line in a flame-spectrum, but have distinctly announced the opinion that in *all* flames the yellow line is due to the presence of sodium, you will perhaps allow me to state the observations I adduced in support of that view, as published in the *Edinburgh Transactions* for 1856.

"One-tenth of a grain of common salt, carefully weighed in a balance indicating $\frac{1}{100}$ th of a grain, was dissolved in 5000 grains of distilled water. Two perfectly similar slips of platinum-foil were then carefully ignited by the Bunsen lamp until they ceased to tinge the flame with yellow light; for to obtain the total absence of yellow light is apparently impossible. One of the slips was dipped into the solution of salt, and the other into distilled water, the quantity of the solution adhering to the slip being considerably less than $\frac{1}{20}$ th of a grain, and both slips were held over the lamp until the water had evaporated. They were then simultaneously introduced into opposite sides of the flame, when the slip which had been dipped into the solution of salt invariably communicated, to a considerable portion of the flame, a bright yellow light, easily distinguishable from that caused by the slip which had been dipped into pure water. It is thus proved that a portion of chloride of sodium weighing less than $\frac{1}{1,000,000}$ th of a grain, is able to tinge a flame with bright yellow light; and as the equivalent weights of sodium and chlorine are 23 and 35.5, it follows that a quantity of sodium not exceeding $\frac{1}{2,500,000}$ th of a troy grain renders its presence in a flame sensible. If it were possible to obtain a flame free from yellow light, independently of that caused by the salt introduced in the experiment, it is obvious that a greatly more minute portion of sodium could be shown to alter appreciably the colour of the flame. It therefore follows that much caution is necessary in referring the phenomena of the spectrum of a flame to the chemical constitution of the body undergoing combustion. For the brightest line in the spectrum of the flame of a candle—the yellow line R of Fraunhofer—can be produced in great brilliancy by placing an excessively small portion of salt in a flame, in whose spectrum that line is faint or altogether absent. The question then arises whether this line in the candle flame is due to the combustion of the carbon and hydrogen of which tallow is chiefly composed, or is caused by the minute traces of chloride of sodium contained in most animal matter. When indeed we consider the almost universal diffusion of the salts of sodium, and the remarkable energy with which they produce yellow light, it seems highly probable that the yellow line R which appears in the spectra of almost all flames is in every case due to the presence of minute quantities of sodium. The view which would attribute a great portion of the light of the envelopes of flames to the adventitious presence of minute traces of foreign matter, may possibly serve to explain certain anomalous diversities of colour which are observed in the envelopes of flames arising from the combustion of the same elements. Thus tallow, coal-gas, anhydrous alcohol,

and weak spirit of wine all contain the same combustible substances,—carbon and hydrogen; yet the envelope of the flame of a candle is bright yellow, that of a coal-gas flame is purple, and those of strong and weak spirit differ greatly in luminosity*.”

I have also observed that “if the air be dusty from any cause,” yellow light appears abundantly in the flame of the Bunsen lamp†; and “I have found that the column of heated air arising from the flame of a spirit lamp with a salted wick is most energetic in communicating yellow light” to the flame of the Bunsen lamp‡. I have likewise ascertained by repeated experiments, that platinum, by mere exposure to the air, speedily acquires the property of tinging flame with yellow light.

The use of a collimator, represented at the letter B in the woodcut accompanying Professors Kirchhoff and Bunsen’s paper, is attended with much convenience and advantage in spectrum experiments where angles are to be accurately measured. This method of observation was devised by me some years ago; and a detailed account of it will be found in the Transactions of the Royal Society of Edinburgh, vol. xvi. and vol. xxi. p. 421.

I am, Gentlemen,

Your obedient Servant,

Ardchapel, August 3, 1860.

WILLIAM SWAN.

XXII. *Considerations in reference to M. Rose’s Memoir on the different conditions of Silicic Acid.* By M. C. SAINTE-CLAIRE DEVILLE§.

IN his valuable memoir “On the different conditions of Silicic Acid||,” Prof. Rose discusses a subject which interests both the chemist and the geologist, and upon which I wish to offer certain considerations. Before, however, going into the question, I wish to prevent the confusion which might arise from a passage in M. Rose’s memoir, in which, citing my researches, he appears to attribute to me merit to which I am not entitled.

He says, “Gaudin and St.-Claire Deville have succeeded in melting in large drops, and drawing into wire, considerable quantities of crystallized quartz, in other words, silicic acid having a density of 2.6. Subsequently Deville succeeded in melting as much as 30 grms. of silicic acid.”

1. I wish to observe that I have no claim whatever to the processes by which M. Gaudin has been able to obtain a very elevated temperature in a few minutes, in order to melt and draw

* Trans. Roy. Soc. Edinb. vol. xxi. part 3. pp. 413, 414.

† Ibid. p. 413.

‡ Ibid. p. 424.

§ Translated from the *Annales de Chimie*, vol. lix. p. 74, by Dr. Atkinson.

|| Phil. Mag. vol. xix. p. 32.

quartz, corundum, &c. I have simply borrowed the apparatus, and the obliging assistance of that ingenious experimenter.

2. That where the name of Deville is mentioned a second time, it refers to my brother M. Henri St.-Claire Deville, who has been able to obtain enormous temperatures by other methods than those of M. Gaudin.

I may be further permitted to mention, in reference to the historical part of the question, that I was the first to show that *quartz-glass* has a density of 2.22, which is therefore 0.17 lower than that of the crystalline quartz from which it is derived.

I insist upon this fact; for it is by no means isolated. For me, its establishment is connected with a series of previous researches on the properties which rocks and silicated minerals acquire when they are melted and then rapidly cooled, but more especially on the properties which sulphur assumes under these conditions.

Lastly, I believe that these singular properties of quartz, felspar, &c. may have exercised considerable influence on the conditions under which granite is formed, and that, hence, it is necessary to take them into account when the origin of this rock is discussed.

For some time I have called the attention of scientific men to these peculiar phænomena of *temper*, and to the abnormal distribution of heat which seems to prevail in the interior of one and the same body.

The *superfused* substance holding for a greater or less time a quantity of heat larger than it should normally possess, retains, although in the solid form, the properties of a liquid: in certain cases it retains softness and plasticity, and in all an amorphous and vitreous structure, a greater solubility, and a less considerable resistance to chemical agents. Another portion (the external pellicle, the *tempered* exterior) retains, on the contrary, a lower latent heat, and presents different properties.

This view, whether it be adopted or not (and there are many facts which might be cited in its support,—the well-known experiments of M. Mitscherlich and of M. Regnault, my own researches*, the researches of M. Favre†, &c.), led me to the discovery of *insoluble sulphur*. I first prepared this body by the direct tempering of sulphur; and by the application of this principle I was led to think that *flour of sulphur* being obtained by rapid cooling, and in a minute state of division, must have a larger tempered surface, and consequently the largest quantity of insoluble sulphur,—which was actually found to be the case.

* *Comptes Rendus*, vol. xxv. p. 857; vol. xxvi. p. 116; vol. xxxiv. pp. 534 and 561; and *Annales de Chimie et de Physique*, vol. xlvii. p. 94.

† *Ann. der Pharm.* vol. xxiv. p. 344.

Slow cooling, or to speak more generally, a slow and gradual mode of formation, allows the molecules to become associated under circumstances in which each takes with it the normal quantity of heat; from this a stable equilibrium results, at any rate under the actual conditions of temperature. In the case of sulphur, for example, *octahedral* or *normal sulphur* is formed of 2.07 spec. grav., which is quite stable at ordinary temperatures; while the two others, *soft* or *vitreous sulphur* and *insoluble sulphur*, become more or less transformed into octahedral sulphur, either spontaneously or by simple actions of contact.

It must also be added that the fourth condition, *prismatic sulphur*, whether prepared by fusion, or crystallized from a hot solution of ether, alcohol, chloroform, or benzole, &c., corresponds to a condition in which the molecules, although possessing more heat than the normal quantity, maintain their equilibrium a certain time, and are then rapidly changed below a certain temperature. But the curious part of this is, that this temperature is not constant, but varies with the nature of the solvent, and probably also with other circumstances; so that this phænomenon of transformation does not appear to be connected with a fixed temperature which the substance obtains on cooling, but rather with the quantity of heat which the liquid had assimilated and had abandoned at a certain moment. Lastly, it may be concluded from my experiments*, that when sulphur is heated in an open vessel, it appears to pass successively through different conditions of equilibrium; sometimes it strongly retains heat, at others it rapidly parts with it: this seems to constitute a sort of rotation, of which the prismatic condition, which commences at about 109 degrees, is only the first term.

Among the very interesting researches which many chemists have lately undertaken on the properties of insoluble sulphur, that of M. Cloez has greatly struck me. Some years after I had described the sulphur made insoluble by tempering, MM. Fordos and Gelis announced that they had obtained an analogous and probably identical sulphur by the action of water on sulphide of nitrogen or chloride of sulphur, &c. M. Cloez, by varying the conditions of the experiment, has produced indifferently soluble or insoluble sulphur; and he adds†, "It is thus shown that chloride and bromide of sulphur produce insoluble sulphur by a rapid decomposition, and soluble sulphur by slow decomposition,"—an evident confirmation of what I had previously announced.

I have also been struck with M. Debray's curious experiment‡, in which he reproduces *at will* prismatic sulphur in bisulphide

* *Ann. de Chim. et de Phys.* 3 sér. vol. xlvii. p. 110.

† *Comptes Rendus*, vol. xlvi. p. 496.

‡ *Ibid.* vol. xlvi. p. 576.

of carbon (which neither I nor M. Pasteur had been able to do), provided that the liquid, *heated at least to 80 degrees*, undergoes a sudden cooling.

Without further insisting on these ideas, which more and more confirm my researches made during the last fifteen years, may they not be applied to the consideration of the subject which H. Rose has treated in his memoir? As he justly remarks, the capital fact is that which results from a research of M. Schaffgotsch* in 1846; that is to say, that quartz in its natural mode of occurrence is in two very different molecular conditions. The one—the crystalline state—has a normal density of 2.65; the density of the other does not differ much from 2.2. This notion has been very happily developed by M. Rose, who has shown that these two natural varieties of silica have not the same chemical affinities. The one variety, comprising vitreous quartz, compact quartz, and silex, resists hydrate and carbonate of potash very energetically; while the other variety (opal, whether calcined or not, infusorial silica, &c.) is strongly acted on by these agents. I had also observed in regard to the *vitreous quartz* obtained by temper, that not only had it a density of 2.2, like the natural amorphous varieties, but also that, like them, and perhaps even better than them, it dissolved most readily and almost completely in alkaline lyes.

This latter fact is closely connected with the preceding explanation; doubtless the very small residue left unattacked by the solution of amorphous quartz in alkaline lyes is comparable to that which the solution of the external coating of pieces of soft sulphur, or even of prisms obtained by fusion, leaves in bisulphide of carbon.

As to natural opal, it is evidently formed, as Beudant and Klaproth have long ago shown, from the gelatinous silica which is found still soft in the altered Hungarian trachytes, and the gradual hardening of which may be observed in the suites of specimens in collections. Ebelmen's ingenious researches have shown facts entirely analogous in the silica rapidly precipitated from artificial solutions. "On the other hand," says M. Rose, "the silicic acid of a higher density which constitutes flint, chalcedony, and crystallized quartz, might be formed from a perfect solution of silicic acid. By the *slow concentration* of this solution, crystallized quartz might be produced."

Thus, in almost all cases, silicic acid of 2.2 spec. grav. may be considered as resulting from a sudden cooling or rapid precipitation †,

* Poggendorff's *Annalen*, vol. lxvii. p. 147.

† Silica of organic origin, as that of the infusoria, is doubtless formed under special conditions, which cannot be absolutely assimilated to purely inorganic actions, and in every respect deserves a separate study.

while silicic acid of spec. grav. 2.65 would in all cases be the product of slow and gradual actions.

The concurrence of the two facts may be readily understood; gelatinous silica, cemented by solidified silica, would have a density intermediate between the two.

M. Rose rightly remarks that none of the natural varieties of quartz of the density 2.2 appear to owe their molecular state to fusion. And as the quartz fused in the laboratory in small pieces is always vitreous, and of this low specific gravity, it must be concluded that the quartz of granite has never been in a state of igneous fusion. With regard to the felspar of granite, a similar conclusion must be drawn from the fact that the experimenters who have succeeded in melting this body have never been able to obtain it crystallized, but always in a state of glass.

All geologists will understand the hesitation I feel at following this able chemist in his discussion on the origin of granite. Not that the question appears to me as it does to him, *as a barren one*. On the contrary, I am convinced that it is as fruitful and as well deserving attention as any of those which are connected with the habitable globe.

I cannot, however, but remark that the foregoing conclusions cannot be legitimately deduced from the negative facts observed in the laboratory. For my own part, I confess that if any one had thus argued when I passed over the lava of Arso, which burst out in 1308 in the Island of Ischia, and which is full of the well-known beautiful felspars, it would have been difficult to convince me that nature has not been able to form felspar* by igneous action.

I will say as much for the micas of Vesuvius, and of those of the lava and scoria of the Laacher See; and, supposing even that they did not contain a trace of fluorine, an element so variable in the micas of granite, nor the small quantity of water the existence of which is very problematical in most cases, I should boldly conclude that if the mica has in the above cases an eruptive origin (and for a geologist there cannot be the slightest doubt), the micas of granite have also been formed in the same manner.

This kind of negative argument cannot therefore be used to combat the eruptive origin of two of the elements of granite. There remains quartz: M. Rose (after many able chemists, and particularly MM. Fuchs and Bischoff) offers several objections to its eruptive origin.

The first depends upon the negative results of the laboratory—that a glass, and not a crystalline quartz, is always obtained by

* By felspar I understand orthose, which is the prevalent felspar in granite. What I say is much more true for oligoclase, and more especially for Labradorite, which is so abundantly found in modern lavas.

fusion. But it is scarcely necessary to dwell upon the absolute difference of physical conditions between a few grammes of this quartz fused and immediately cooled, and masses of the substance imbedded in a granite block. If it be objected that in no case in nature, not even in granitic veins of small dimensions, which therefore are in conditions most resembling those of the laboratory, has quartz been found in a vitreous condition, it might be answered that we are entirely ignorant of the limits to the stability of quartz in this condition. If, as I think, the vitreous state is an abnormal condition for all bodies capable of acquiring it*—a state in which the molecules are retained at too great distances from each other by an excess of latent heat,—each of them ought to tend to still further removal, in order to obtain position of stable equilibrium, which is in fact the crystalline state. This is well seen in sulphur, arsenious acid, &c. I have shown that soft sulphur left to itself, although gradually approaching octahedral sulphur nearer and nearer, had not become changed into that state even after several years: this was perceptible from the differences of density. But inasmuch as there are necessary limits to the operation, we see that by prolonging the experiment as long as we like, we could never affirm that the change was complete. Can it not be conceived that the quartz of granite (originally vitreous like sulphur, like vitreous arsenious acid, like ordinary glass) has undergone, after a long time and under favourable conditions, a molecular change which has altered it into the crystalline state without imparting to it any external geometrical form?

The beautiful experiments in which M. de Sénarmont and M. Daubrée have produced quartz in small but perfect crystals or in concretionary masses, are well fitted to explain the origin of quartz in veins, in geodes, and even in stanniferous masses; in short, to use an idea of the first importance introduced into science by M. Elie de Beaumont†, they explain the presence of quartz in all cases in which it is connected with the phenomena

* I have shown (*Comptes Rendus*, vol. xl. p. 769) that the properties of *superfusibility* by temper are far from belonging to all bodies. The metals which I have examined (lead, zinc, and bismuth) have not exhibited it; nor has chloride of sodium. It is remarkable that alumina, a body resembling silica in many respects, is entirely free from it. While quartz presented the greatest difference in density between the crystallized and the vitreous state, fused and tempered corundum had exactly the same density as natural corundum.

† "Note on Volcanic and Metalliferous Emanations" (*Bulletin de la Société Géologique de France*, 2 sér. vol. iv. p. 1250). M. Bernhard Cotta, in giving a complete translation of this memoir (*Gangstudien*, 4th part, 1850), has performed a real service for those persons on the other side of the Rhine who are interested in these questions.

of *eruptivity in the manner of sulphur*, at the limit of which we find the mineral waters and their sources. But these experiments do not directly apply, in my opinion, to the case in which quartz, like that of the granites, is a substance *eruptive in the manner of the lavas*. For the silica obtained by these philosophers has always been crystallized; and it is superfluous to show that it would be false to assume that crystallized silica could not be obtained in any other way.

Supported by the possible and even probable superfusion of quartz, M. Fournet has endeavoured to explain how that this body in a great many cases, more especially in granite, solidified after other substances which were more fusible than itself. From well-known facts, M. Elie de Beaumont has shown* that there was nothing improbable in such a hypothesis. If Mr. Faraday, working with small quantities and under unfavourable conditions, could retard the solidification of sulphur by nearly 100 degrees (its fusing-point being 109°), what is there astonishing in the assumption that quartz, which perhaps melts at 2000 to 2500 degrees, could be maintained in the soft state as low as 1000 to 1200 degrees, and that under conditions which must be assumed to be favourable? I will add that M. Fournet's opinion, which, when he published it, was only an ingenious hypothesis, seems to me to have become much more probable since it has been found that pure quartz is eminently well adapted to form a glass, that is to say a superfused substance, and that up to the present time it is the substance which presents the greatest difference between its density in the crystalline, and its density in the amorphous state. I therefore cannot see in that an absolute objection to the eruptive origin of granite.

"But," the able author whom I contend with would remark, "how can it be conceived that minerals which, like felspar, only contain 60 to 65 per cent. of silica, or like mica, which is still more basic, could be separated from a fused mass in which the granite is in excess? Evidently this body, which at this temperature acts powerfully on the bases which constitute these minerals, would not have allowed those substances to crystallize out without combining with them."

To refute this objection, it is simply necessary to assume with M. Delafosse, that, in reference to minerals of igneous formation, silica plays a part analogous to that which water plays in reference to substances formed in it. In the latter case definite hydrates are formed, frequently poor in water. There is thus a class of phænomena in which affinities find their limits. Further, M. Sénarmont's ingenious experiments prove that elevation of temperature always produces a tendency to dehydration, even in

* *Loc. cit.* p. 1305.

a liquid medium. For instance, gelatinous silica entirely loses its water, and becomes deposited as crystalline quartz; and a solution of sesquichloride of iron decomposes into hydrochloric acid and anhydrous sesquioxide.

My brother*, in his researches on the phenomena of *disso-ciation*, has cited still more curious facts: oxygen and potassium not only do not combine, but at a high temperature cannot remain combined. In the presence of facts like these, it is not astonishing that silica in a fused state should act as *mother-liquor* to the felspar, only combining with the other elements within limits appropriate to the physical and chemical properties of the granitic bath.

One of the objections which M. Rose raises against the eruptive origin of granite, depends on the remarkable properties of the so-called *pyrognomic* minerals. M. Scheerer, who has profoundly studied this class of phenomena, has perfectly well seen that to found on them an absolute argument against the eruptive origin of granite is to exaggerate their importance. He has even been led to conclude that the solidification of the rock has taken place under special physical conditions. Indeed M. Rose himself, who first presented this objection, does not seem to attribute to it any considerable value; for he adds†, "It may be assumed that these minerals, more especially gadolinite, are produced by fusion while associated with granite, but that by the prolonged action of the atmosphere, of water, of an elevated temperature, and other influences they have changed this condition."

If, as M. Rose observes, such a hypothesis is not opposed to the ideas which he endeavours to enforce in his memoir, still less is it opposed to those which I advocate.

But if we in turn examine the defence of the partisans of the Neptunian origin of granite‡, how are we to explain the division of the different acid and basic elements, between the micas and the felspars? Why should there be formed at one time two micas? and more especially two felspathic minerals having two formulæ essentially different, orthose and oligoclase? Evidently nothing that takes place in our laboratory solutions can give the key to these natural phenomena. Let us admit, then, that between the different elements of the rocks there

* *Comptes Rendus*, vol. xlv. p. 489; *Phil. Mag.* vol. xvi. p. 516.

† *Phil. Mag.* vol. xix. p. 39.

‡ I use the expression *Neptunian origin* employed by M. Rose, and not that of *aqueous origin*. The latter, in fact, cannot be the antithesis of *plutonic* or *eruptive* origin. I shall show further, that not only is water present in eruptive phenomena, but there is actually no eruption in which it does not play an important chemical part.

is a very singular and complicated equilibrium, an idea of which may in some measure be obtained if we represent it by a certain number of indeterminate equations, for the solution of which one condition is wanting. But this condition must be sought for in the study of the phænomena of igneous fusion.

M. Rose justly recommends geologists not to advance, without weighty and powerful motives, hypotheses which are in contradiction with the known laws of chemistry. But may not geologists well recommend chemists, who bring to these difficult questions the aid of their special knowledge, to submit their solutions to the control of natural facts? However ingenious these solutions may be, whatever support they may find in laboratory experiments, the first and indispensable condition of their adoption by a naturalist would be their agreement with observed facts.

But there are fundamental analogies, which no geologist since Hutton has refused to recognize, between the oldest granite and the most recent eruptive rocks, the volcanic lavas. This will be readily admitted by any one who has seen, or has even merely read a description of a flow of Vesuvius or Etna, of the amphibigenic or doleritic veins of the Somma or of the Val del Bove; the trap-dykes and melaphyr of Scotland or of the Palatinate; the *Elvan* of Cornwall; the quartziferous porphyry of Saxony or of Morvan; lastly, some of the granitic veins so frequently met with in the latter places, and in innumerable others.

It is true that these general analogies of formation, the establishment of which was one of the triumphs of the end of the last century, do not amount to an absolute similitude: the mineral elements, for example, vary from one group of rocks to another. The presence of quartz, especially, has not been observed in any true flow of lava, but it exists in the trachytes of the Siebengebirge and of Mont-Dore: I have found it in the doleritic rocks of Guadaloupe; some considerable tracts of this latter island, and of Martinique, are covered with a reddish earthy crust which is full of innumerable fragments of transparent quartz, arising, like it, from the decomposition of the volcanic rocks. It likewise occurs with Labradorite in the doleritic trachyte which constitutes the cone of the Soufrière. And since the density of this quartz is 2.65, we cannot attribute to it an origin analogous to that of the opals of Hungary. Thus step by step we succeed in finding quartz in formations which, by their age, by their nature and mode of occurrence, are nearest to the lavas which have flowed under our own eyes. It is impossible to avoid establishing a comparison between the conditions under which are formed the crystals an agglomeration of which constitutes the rock, more

especially when we find that the predominant mineral in the Vesuvian lavas is amphotigene, a substance resembling quartz in its infusibility. Can it be doubted that in such a case we witness the more or less rapid transformation of a melted or viscous magma (certainly, as M. Durocher remarks, more fusible than the most refractory of the minerals which could be separated from it) into a solid rock almost entirely composed of crystalline elements? Truly it is pre-eminently an eruptive act; for it is one by definition. But at the same time that these streams of *liquefied earth*, as M. Humboldt calls them, escape from the volcano, bodies of quite a different nature are also disengaged,—alkaline and metallic chlorides, a small quantity of sulphates and phosphates, then at various intervals of time and place hydrochloric, sulphurous, hydrosulphurous, and carbonic acids, but more especially aqueous vapour. The latter rises from the lava many years after its eruption; and any one who has followed day by day, step by step, the chemical phenomena of lava in motion, can have no doubt as to the origin of the water. Like other substances, it forms an integral part of the magma; and like them it separates at a given moment, in proportion as the internal reactions of the incandescent mass are affected*.

As far as I know, M. Elie de Beaumont, in the memoir previously quoted, was the first to establish this kind of preliminary solution of water and of salts in incandescent lavas. He rightly refers it to certain phenomena which are readily reproduced in the laboratory, such as the spitting of silver, the experiments on the spheroidal state of bodies, &c. It may be added, that the properties of obsidian and its artificial transformation into pumice furnish also an indisputable proof of this fact.

But these gaseous exhalations which accompany lava do not disappear without leaving some traces. The more or less complicated reactions which are set up between their elements and those of the rock or of the atmosphere produce the chlorides of iron, of copper, of cobalt, of lead, specular iron, and protoxide of iron, oxide of copper, alkaline sulphates, sal-ammoniac, apatite, which, in varying quantities, doubtless impregnate all modern lavas.

If, starting from the eruptive phenomena which we may daily witness, we pursue the analogy to the older rocks, to the granites, is it possible not to admit, with M. Elie de Beaumont, the existence of *granitic fumaroles*, which separating from the

* I am strongly inclined to think that the powerful columns of aqueous vapour which cause the explosions, which form, as it were, the first act of all great eruptions, are only emanations from lavas below: they accumulate until their expansive force bursts through the solid crust of the crater, and projects it into the air.

original bath, have deposited these oxides of iron, of tin, of titanium, these sulphides of molybdenum, topazes, tourmalines, even phosphate of lime—in a word, this pleiad of bodies which Humboldt has called the *penumbra of granite*, and on the formation of which Daubrée's experiments have thrown so much light?

Here, in fact, we meet with quartz. It is the most usual and most abundant element of these deposits. It constitutes, so to speak, the substance on which these varied minerals form a sort of embroidery. But there can then be no doubt as to its origin. It is a result of secretion. It is the product of molecular actions; and if, as everything tends to show, water has played a part in these actions, this water must have formed an integral part of the granitic magma, and separated in the state of fumarole, carrying with it the other volatile elements of these reactions. Here we have physical and chemical circumstances altogether peculiar; we have a key to the explanation, furnished both by observation and analogy, and which it is impossible not to take into account.

If to the physical properties of *superfusible* bodies, on which I have dwelt at the commencement of this article, we add this intimate association, this kind of combination, which it is impossible to deny, and which takes place under the influence of particular and unstable causes between the mineral substance and water or other volatile substances, why give up the explanation of the formation of quartz and felspar by purely eruptive phenomena? Is there not an intimate association between the two classes of facts? Is not the faculty which a viscous body possesses of assimilating liquids or gases, related to the abnormal assimilation of heat which constitutes superfusion in viscous bodies? If I am not deceived, so long as these delicate points of molecular statics are unexplained, it will be impossible to affirm anything as to the present question.

Before seeking to explain the formation of granite, let us try to account for what takes place before our own eyes in a solidifying lava. When we know something as to what influences the division of the elements between the small number of minerals formed, when we have some idea of the part played in these curious phenomena by the substances which gradually escape in the gaseous form, it will be time to attempt the question on its more complicated and difficult side. Only then shall we be able to reason, without too great chances of error, on the cause which, at the most ancient epochs of the globe, and under physical conditions different perhaps from those of the present, has produced, from the innumerable solid, liquid, and gaseous elements of which we find the trace, the definite and stable equilibrium from which granite has resulted.

XXIII. *On a new Theoretical Determination of the Velocity of Sound.* By the Rev. S. EARNshaw, M.A., Sheffield.

[Continued from p. 41.]

The Triplicity of Sound.

THAT the actual velocity of violent sounds should be greater than that of ordinary sounds, and that the velocity of ordinary sounds should be greater than was found by Newton, are results which theoretically depend only on the hypothesis of finite intervals as distinguished from that of continuity; but that the numerical value of the velocity of ordinary sounds should be exactly what it is, is a circumstance which depends also on the particular law of force according to which the molecules of the atmosphere act on each other. As far as I know, this law has not hitherto been experimentally determined; it was open to me, therefore, to assume any law to which there should be no *à priori* objection. It is of course essential that the assumed law must give a result agreeing with experiment; and it might have happened that no *simple* law of force could have been found which would give a result agreeing with the experimental velocity of sound. But not only did a simple law present itself capable of doing this, but the law which it was found necessary to assume is seen to be the *lowest* power of the inverse distance which the mathematical expressions themselves would permit us to try. I take this to be a strong presumptive evidence in support of the theory advanced; and I shall not hesitate to employ the modified forms of the preceding equations which this assumed law of molecular action will enable us to introduce,—merely observing, in doing so, that the physical results obtained do not really depend, as to their character, on the assumption of this particular law, but follow from the more general equations also; only, the mathematical expressions are by this means rendered shorter and more manageable, and consequently the results more obvious to the general reader.

14. Assuming, therefore, $f'(z) = \frac{C}{z^4}$, the last equation of art. 6 gives

$$\sqrt{\mu} = \frac{\pi^2}{h} \cdot \sqrt{\frac{Cm}{90}};$$

and by treating equation (7) in the same way we find

$$v = \frac{\sqrt{15\mu}}{\pi} \cdot \left(1 - \frac{\theta}{\pi}\right) = V \left(1 - \frac{h}{\lambda}\right). \quad \dots \quad (10)$$

In the case of all ordinary sounds, h is indefinitely small compared with λ , and the last term (as stated in art. 5) may be neglected. But turning our attention to all possible sound-waves, the last term may not be neglected, but will become more sensible in proportion as λ is less and less. This term leads to results of great theoretical importance. For if we eliminate λ between equation (10) and the reduced equation for k (3), we obtain

$$\frac{V}{2\pi} \cdot hk = Vv - v^2, \quad . \quad . \quad . \quad . \quad (11)$$

which expresses the exact relation between k and v in the case of every sound of the non-violent class as defined in art. 10.

15. The symbol k is proportional to the number of vibrations per second executed by any given particle, and, being so, may be taken as a measure of the pitch of the note sounded. This being agreed upon, the equation just found, being a quadratic in v , shows us that there are *two different velocities with which a given musical note may be transmitted*. The equation shows, moreover, that the sum of these velocities is always equal to V ; and hence, in the case of such musical notes as are not too high for audibility by human ears, one of these velocities must be indefinitely small, since the other differs (see art. 5) insensibly from V . And, further, the length of the wave by which sound is rendered audible to human ears is always large compared with h , the distance between two neighbouring particles of air; but the length of the second wave, by which the same note is transmitted, is always extremely minute, never exceeding $2h$; and consequently this wave is too minute and feeble as to quantity of momentum to affect such ears as ours; and if audible at all, can be so to none but the most minute creatures whose existence has been revealed to us by the microscope. And if any doubt should be entertained as to the existence of this minute order of waves on account of their having never been perceived by the experimentalist, let it be remembered that equation (11) is exact, and that there is consequently no more ground in theory for believing in the existence of those finite waves whose existence is admitted, than of these extremely minute waves whose theoretical existence is now for the first time pointed out. Equation (11) indicates that one kind of wave has in theory as real an existence as the other. I take it therefore as proved, that *a musical note of any pitch is transmissible with two different velocities*; and that *there are two waves for every note*.

16. Yet there is one *essential* difference between the two waves corresponding to the same note, which is indicated by the following properties, viz. that the length of the ordinary wave in-

creases as k diminishes, while that of the other wave *diminishes* under the same circumstances. An ordinary wave is therefore essentially different from the other; and the difference between them may be made to rest on the properties here pointed out. The second kind of wave is not and cannot be a wave of the first kind. They may also be distinguished by this property—that the length of a wave of the ordinary kind is always greater, and that of a wave of the minute kind always less, than $2h$, the distance between two particles of the air.

17. Equation (11) shows that for any value of v there is a corresponding value of k ; but as in the preceding investigations k was taken to be essentially positive, the only admissible values of v are those which lie between zero and V . And of these, those which lie between V and $\frac{1}{2}V$ belong to the class of ordinary gentle sounds, and those which lie between $\frac{1}{2}V$ and 0 to the class of minute gentle sounds.

18. Equation (8) teaches us that when the sound is of the type which we have termed violent, any value of v is possible between V and ∞ . And hence the conclusion with respect to possible velocity is, that *the atmosphere is capable of transmitting sound-waves with any degree of velocity from zero to infinity*. But it must be noticed that this range of velocity divides itself into three essentially distinct portions; viz. from 0 to $\frac{1}{2}V$, from $\frac{1}{2}V$ to V , and from V to ∞ , corresponding to the three distinct kinds of waves—*minute, ordinary, and violent*: and the two former belong to the *circular*, and the last to the *exponential type*. And with the same value of k there may coexist three distinct waves—one of each of these kinds—all *propagated with different velocities*. This is what is meant by the title at the head of this communication, *the triplicity of sound*.

19. The form of equation (11) shows that k admits of a maximum by the variation of v , viz. $\frac{\pi V}{2h}$. (It is easily seen that it is the velocity corresponding to this value of k which separates the two classes of gentle sounds.) Hence we perceive that *the particles of the atmosphere cannot execute more than $\frac{V}{4h}$ free vibrations per second*.

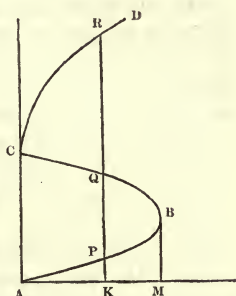
20. Much additional light will be thrown on the results arrived at in the preceding articles with respect to the wave-properties of an elastic medium like the atmosphere, if we present the relation between k and v to the eye by means of curves. There are two types of waves—the *circular* and the *exponential*. In the case of the former the analytical relation between k and v is expressed by the equation (11). We may show this by a curve, if we take the different values of kh for the abscissæ, and

those of v for the ordinates. The curve is evidently a parabola whose latus rectum is $\frac{V}{2\pi}$; it is represented by $A B C$ in the figure. $A C$ is $= V$; and $A M$, the maximum value of kh , is equal to $\frac{\pi}{2} \cdot AC$. The magnitude of the parabola will vary for different elastic media, but $A B C$ will always be a *similar* portion for all media.

In the case of the exponential type of wave, the relation between k and v is expressed in terms of α by the equations (7'); and if a curve be constructed as in the former case, we shall find it to be of the form $C D$, touching at C the line $A C$ produced. If $A K$ be any abscissa, and $K R$ be drawn parallel to $A C$, then there will be *three* ordinates, $K P$, $K Q$, $K R$, representing the three different velocities of the three corresponding waves. The figure therefore represents to the eye the triplicity of sound.

21. $A M$ corresponds to the maximum value of k , which is exceedingly enormous; but in the case of all audible sounds k does not much exceed 20,000, which is probably small in comparison with its maximum value. Hence the part of the curve $C B$ which corresponds to audible sounds lies close to C , where it is obvious the ordinates are all nearly equal, which is the reason why the velocity of transmission of all ordinary sounds is sensibly the same. But since the curve $C D$ has $A C$ produced for its tangent at C , the ordinates which lie near to C will differ sensibly from $A C$ in magnitude; and hence a slight variation of the value of k will be attended with a sensible variation in the value of the velocity of transmission in this case; and we may therefore expect that the velocity of a thunder-clap will be greatly different for small differences of intensity of the electrical discharges.

22. In considering the question of the intensity of sound, we cannot but think it depends chiefly, perhaps entirely, upon the whole momentum which reaches the ear in a given time; and this will chiefly depend in a given case upon the velocity of propagation. Hence the sounds which belong to the whole of the curve $A B$, and to a large portion of $C B$ lying near to B , would probably be inaudible for want of sufficient momentum, even were there no other reason. On the contrary, sounds belonging to $C D$, with a usual amount of force in their genesis, will never be inaudible from this cause. And hence if any method should be found of simultaneously generating the three different sound-waves corresponding to any value of k within the limits of audi-



bility, and if the genesis should be so managed as to begin from $k=0$, and to be capable of changing to fresh values of k , then, from the beginning, sounds corresponding to parts of the curves C B and C D about C would be heard together; but as the genesis went on, the sounds corresponding to the curve C B would get less and less intense, and those corresponding to C D more and more intense; and the former would gradually become inaudible, and the latter too loud to be borne. Those corresponding to A B, if perceived at all, would be most sensible for parts near to B.

23. Hence the portions both of C B and C D which are applicable to the hearing powers of human beings are very limited, and begin from C in both cases. But it has been supposed by Wollaston that the Grylli take up the faculty of hearing where we lay it down; and there may be animals which take it up where the Grylli lay it down; and it is thus conceivable that the whole curve C B is portioned out among different orders of animals from man to the minutest insect. But the curve A B, for the reason pointed out in art. 16, may not belong to waves which affect the organ of hearing at all, but may appeal to some other sense, of which we are probably not possessed, but which is suited to the wants of the minute creatures which the microscope has difficulty in revealing.

The Analogy of Sound and Light.

24. It is admitted on experimental grounds that a certain degree of analogy exists between sound and light; and it has been considered allowable to consider the latter as being due to the undulations of an elastic medium consisting of particles separated by finite intervals. Hence the preceding investigations and results, after the necessary alteration of numerical constants, will apply to the æthereal medium. There is therefore a maximum limit to the number of free vibrations which the particles of the æthereal medium can execute per second; and every number from zero to this limit is possible. For this medium also, as for air, there are two types of wave-disturbance—the circular and the exponential; and three distinct classes of waves, corresponding to three different velocities of transmission for every value of k . There is, in fact, a triplicity of light; and every degree of velocity from zero to infinity is possible for light as for sound. The figure given in art. 20 preserves the same proportions for all media, and will therefore serve for light: the only question that occurs is, what part of it represents *sensible* light? Sensible sound, as we have seen, is represented both in the curve C B and in C D by a small portion of each near to C; but which parts of the three curves represent sensible effects in the

case of light? Do the three curves all represent light? May we not assume that one of them, as CD , represents heat; another, as CB , light; and the other, AB , actinism? And if this appropriation of the three curves be allowed, what part of CD represents *sensible* heat, what part of CB *sensible* light, and what part of AB *sensible* actinism? It seems reasonable to suppose that heat will, by means of delicate instruments, be sensible from C ; but the moderate variation in the lengths of the waves of sensible light renders it probable that the portion of CB which represents sensible light will commence at some distance from C ; for the length of a wave is represented by the tangent of the angle subtended at A by the ordinate which represents the velocity of its propagation. As to actinism, but little is known at present to guide us in determining where the portion of AB lies which represents its sensible effects. It seems almost certain, however, on account of the extreme smallness of the ordinates of AB , which represent the velocity of its transmission, that only the part of AB which lies towards B will represent effects of actinism which come from the sun. Those represented by the part of AB which lies towards A travel too slowly to reach us, and must be, besides, extremely feeble. Judging then on these principles, we conclude that, roughly speaking, the part of CD *beginning from* C represents sensible heat; the *middle of* CB sensible light; and the part of AB *near to* B sensible actinism. And this being so, it would seem that heat, *if moderate*, ought to be found at the red end of the spectrum, and actinism at the blue end.

25. If the process by which the temperature of a body is raised be such as to give rise to each of the three sets of waves which correspond to a set of values of k , and if, further, the process be such as to produce, as it goes on, larger and larger values of k , it is easy to see, after what was said in the last article, that the *heat* is sensible from the beginning; but not so the light, nor the actinism. But as the temperature rises, and k takes larger values, we approach the middle of CB , so that by-and-by the light will become sensible; and this will first happen to the *red* light. As the process goes on, the other colours will be added; and the body, from appearing of a red heat, will approach to white heat; and ultimately the actinism will have become sensible also. There is nothing forced in this explanation. It follows naturally from the principles previously laid down, and the general results obtained: and thus *the connexion between light, heat, and actinism is shown to be essentially due to the nature of the elastic medium in which they take place*, and to follow necessarily from the hypothesis of finite intervals laid down at the commencement of these investigations.

There yet remain several results of great interest, which follow from what has been done in the preceding articles ; but they must be deferred for a while, as at the present time other engagements press upon me.

Sheffield, July 25, 1860.

XXIV. *Photographic Observations of the Solar Eclipse, July 18, 1860.* By JOHN SPILLER, F.C.S.*

ON the occasion of the late solar eclipse, July 18th, the equatorial telescope belonging to the Royal Artillery Institution, Woolwich, was through the kindness of Captain E. J. Bruce, R.A., placed at my disposal, and an opportunity thus afforded me of attempting the photographic representation of the solar disc as it appeared from this station under the several phases of partial eclipse. The successful termination of the day's labour, resulting in the production of twenty-three photographic impressions of the phenomenon in its consecutive phases, appeared to justify a descriptive account of the general arrangement of the apparatus and the mode of operating, which it is hoped will possess some degree of scientific interest.

From previous experience in connexion with the similar event of March 15th, 1858†, when the same telescope was employed, it was found necessary to modify on the present occasion the disposition of apparatus then adopted ; and particularly to restrict the admission of the solar rays by the substitution of a very much smaller diaphragm for the large aperture then rendered necessary by the unfavourable state of the weather.

The telescope, with its portable stand provided with means of adjustment in altitude and azimuth, was, on the 18th of July, erected in the open air within the enclosures of the Royal Military Repository on Woolwich Common, and immediately contiguous to all the appliances of a well-furnished photographic laboratory. The object-glass, 4 inches in diameter, has a sidereal focus of 77 inches, and gives a representation of the sun's disc measuring, at this season of the year, .7 inch in diameter. For photographic purposes the eyepiece was removed from the telescope, and a small sliding-bodied camera adapted to the end of the tube ; it was then easy to project upon the ground glass a perfectly defined image of the solar disc, using the means of adjustment which the camera afforded for the purpose of obtaining the best optical focus ; and in this plane the prepared

* Communicated by the Author.

† *Vide* Journal of the Photographic Society, April 21, 1858.

photographic surfaces were usually employed, for the chemical and visual foci had previously been ascertained to be very nearly coincident. The aperture of the object-glass was now stopped down until, with a diaphragm of $\cdot 25$ inch, the exposure to the powerful action of direct sunlight was rendered manageable; and in order to secure an easy and sufficiently rapid means of opening and closing this small aperture, a card of about 6 inches square was provided, having cut out from its centre a narrow slit of about an eighth of an inch in diameter and nearly 2 inches in length. This quickly moved by the hand in front of the diaphragm of the telescope lens, limited the period of exposure to a small fraction of a second, and besides made it possible to regulate the interval of time at the taking of each picture according to circumstances at the moment, which were occasionally varying as light fleecy clouds passed over the face of the sun. Mr. Crookes, to whom I am indebted for taking charge of this part of the apparatus, was provided with dark glasses, to enable him, by watching the sun, to select the proper moment and judge the length of time which each plate would require in its exposure; and he agrees with me in preferring this system of operating to the use of a fixed mechanical contrivance for opening and closing the aperture of the lens.

By proceeding in the manner indicated, we endeavoured to restrict the photographic action to the representation of the sun's disc alone, and only in the first and second plates of the series was sufficient time allowed for the highly illuminated clouds around the sun to become imprinted in the camera; although faintly visible sometimes in the field of view of the telescope, their intensity was now so much lowered as not to be copied. It will be evident also from the foregoing description, that no clockwork mechanism was required for the purpose of making the apparatus follow the diurnal motion of the sun.

The glass plates on which the pictures were taken measured $2\frac{3}{4}$ in. by $3\frac{1}{4}$ in., and were numbered at one corner by a scratching diamond; they were cleaned previously and arranged in their order of succession. The precise moment at which each plate received its exposure in the camera was registered immediately; Greenwich mean time being taken the same morning from the Woolwich Observatory, and kept by an ordinary good watch.

The collodion process seemed on all accounts the most available, and was that employed to furnish the negative pictures from which copies on paper have afterwards been printed; and in order to guard against accidents, the silver baths, collodion, and the more important solutions were provided in duplicate, so that no difficulty was experienced in preparing the sensitive plates in rapid succession. The services of two of my pupils in photo-

graphy (non-commissioned officers, Royal Artillery) were found extremely valuable in conducting the fixing and subsequent operations, taking from my hands the plate immediately after the development of the picture.

In viewing the photographs thus produced, it may be stated that several of them exhibit very clearly the position of the solar spots, and that in many cases the lunar mountains are to be seen sharply defined against the bright face of the sun. A passing cloud rendered the "first contact" so imperfectly visible that no opportunity for opening the camera was presented; but the plate representing the moment of maximum obscuration, and the phases both immediately preceding and following, are fortunately amongst the most successful of our photographic results; and further, on comparing the present series with the disc of the sun taken at the period of the former eclipse, March 15th, 1858, in precisely the same apparatus, the diminished size of the image consequent on the eccentricity of the earth's orbit is very strikingly exhibited.

As a concluding remark, I beg to call attention to the unusual magnitude of a group of spots visible upon the sun's disc on the 9th instant. They were readily seen without the assistance of the telescope, as a dark patch situated rather low in the southern hemisphere. I have succeeded in securing three concordant photographic proofs representing their appearance at noon and a little later, which, besides exhibiting the principal group as an aggregate of dark spots disposed generally in a horizontal line, indicate likewise the position of several smaller ones irregularly distributed over the face of the sun, and only visible with optical aid. It cannot be said that any distinct indications of faculæ are afforded by these or the other photographs taken on the occasion of the eclipse.

Royal Arsenal, Woolwich,
August 21st, 1860.

XXV. On the Thickness of the Crust of the Earth.

By the Venerable JOHN HENRY PRATT, Archdeacon of Calcutta.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

I ADMIT that I did use the principle pointed out by Professor Jellet in the note to his communication in your May Number, and in so doing was wrong; and therefore the part of my argument against Professor Haughton's investigation in the *Irish Transactions*, which depends upon that step, falls to the ground.

But this does not affect the objection which I brought forward in two of my communications (May 1859, p. 329, and October

1859, p. 259), viz. that his investigation does not concern the question of fluidity and solidity, but only of densities.

Even if Professor Haughton knew the law of density of the fluid nucleus, and also knew every circumstance regarding the solid portions of which the crust is composed, excepting only the total thickness, his equations would not give any information regarding that thickness. The constants involved in the expression for the law of the fluid density (which must be two at least, except in the impossible case of homogeneity) would still be arbitrary; and his three equations (the two appertaining to the outer and inner surfaces of the crust, and the third given by the value of the mean density) would involve at least four unknown quantities. The expression for the thickness would thus come out a function of one or more arbitrary constants, on which the density of the fluid nucleus depends. The greater this density the greater must be the thickness, and *vice versâ*. The problem, in fact, which Professor Haughton's equations would solve is this: if the materials of the earth follow one or more known laws of density (for the density of the crust may be discontinuous) down to a certain depth, and another known law below that to the centre, what must that depth be that the total mass may be what we know the earth's mass and dimensions to be? He introduces no physical principle into the problem characteristic of the different properties of a solid and fluid mass, such a principle as that which Mr. Hopkins introduces, viz. that the fluid parts pressing against the solid shell give it a precessional motion, which is a matter for external observation and measurement; or such a principle as this (which would suffice if experiment had determined the law), viz. the relation between pressure and temperature which serve just to produce solidity. Professor Haughton treats the question more mathematically than physically. He shows that without an exact knowledge of the laws we cannot have an exact solution. But this we knew before as a matter of course, and it did not require Laplace's analysis to prove it.

Our ignorance of the exact laws does not preclude our approximating to a solution. We may use laws which *à priori* considerations may show to be highly probable, and the probability of which may be still further increased by the results they lead to.

For example, the law of density, $\frac{Q \sin qa}{a}$, is a highly probable law for the fluid portion. On the hypothesis that the earth took its form from being in a fluid state, the same law, both of density and ellipticity, is highly probable for the solid parts as well as for the fluid. I think that the Table of Deflections of the plumb-line caused by variations of density from the fluid

law (which I have given in the Philosophical Transactions, 1859, p. 768) tends to show that there cannot be any very important departure from this law; otherwise they would be detected by comparing geodetic and astronomical measures of the amplitudes of arcs, especially wherever those measures have been made in countries free from large visible causes of disturbance, such as, in this country, the Himmalayas and ocean.

Mr. Hopkins's solution he professes only to be an approximation—an answer, in fact, to the question, Is the thickness of the earth's crust a *large* or a *small* fraction of the radius? The consideration of the geographical features of this part of the world (the largest mountain mass anywhere known lying on the north, and an unbroken ocean lying to the south of Hindostan) which I have brought forward, is for the same purpose,—not to attempt an exact solution, but to reply to the question I have quoted.

J. H. PRATT.

Calcutta, July 9, 1860.

XXVI. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from p. 143.]

BERTHELOT* has described a new hydrocarbon. He calls it *acetylene*, or quadricarburetted hydrogen. It has the formula $C^4 H^2$, and is the prototype of the hydrocarbons $C^{2n} H^{2n-2}$.

Acetylene is produced whenever olefiant gas, alcohol vapour, ether, aldehyde, or even wood-spirit are passed through a red-hot tube. It is also formed when chloroform acts upon copper at a red heat: it is contained in illuminating gas. Ether yields it in largest quantity.

In order to obtain it pure, it is formed into a compound with subchloride of copper. This compound is identical with a red detonating body obtained by passing the gases arising from the decomposition of alcohol by the electric spark or by heat, into an ammoniacal solution of subchloride of copper. It was discovered by M. Quet, and examined by M. Böttger; but neither of these chemists analysed the gas which is liberated when this body is treated with hydrochloric acid. This gas is acetylene.

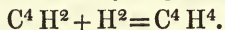
It is a colourless gas, somewhat soluble in alcohol, with a characteristic disagreeable odour. It burns with a very luminous fuliginous flame. Mixed with chlorine, it detonates almost instantaneously, with deposition of carbon, even in diffused light. It cannot be liquified. Its density is 0.92. Its formula, $C^4 H^2$, corresponds to four volumes.

* *Comptes Rendus*, April 1860. *Répertoire de Chimie*, June 1860.

It combines with bromine, with sulphuric acid, with the elements of water, and with hydrogen, to form derivatives parallel to those of olefiant gas.

Acetylsulphuric acid is formed like ethylsulphuric acid, when the gas is agitated for a long time with concentrated sulphuric acid. The mixture, saturated with carbonate of baryta, filtered and evaporated, yields crystallized acetylsulphate of baryta. If the mixture is distilled instead of being saturated, a peculiar liquid is obtained somewhat more volatile than water, very alterable and endowed with a peculiar odour, and soluble in 10 to 15 parts of water. It is *acetylic alcohol*, $C^4H^4O^2$.

Acetylene may be changed into olefiant gas by causing the nascent hydrogen produced by the action of zinc on ammonia to act upon the compound of acetylene and copper. The olefiant gas thus formed is mixed with hydrogen and a little acetylene,



Berthelot* has effected the direct synthesis of iodide of ethyle. A thin sealed tube containing about 20 cubic centims. of a saturated solution of hydriodic acid, is introduced into a long-necked globe of about a litre capacity; the globe is filled with dry olefiant gas and hermetically sealed. The tube in the inside is then broken, and the flask heated for fifty hours to a temperature of 100° .

The flask is then opened, the contents treated with a solution of potash; the product, washed, dried, and rectified, yields about 4 grms. of pure iodide of ethyle.

M. Cahours has published† an extended memoir on organo-metallic radicals. We can only find space for a few of the more interesting results.

When iodide of ethyle acts on alloys of tin and sodium containing 2 to 12 per cent. of the latter, iodide of stanethyle is formed, mixed with a large quantity of an irritating oil smelling like mustard. It boils between 234° and 236° C., and has the formula $(C^4H^5)^3Sn^2I$. It is the *iodide of sesquistanethyle*.

With an alloy of 4 parts of tin and 1 part of sodium two products are obtained. One an oily, limpid, yellowish liquid, which unites directly with oxygen or iodine to form oxide or iodide of sesquistanethyle; it is the *sesquiethylide of tin*, $Sn^2(C^4H^5)^3$. The other is a viscous liquid, stanethyle, SnC^4H^5 .

When this radical is treated with water a thick oil separates, which, when heated, decomposes into metallic tin and a colour-

* *Comptes Rendus*, March 1860.

† *Annales de Chimie et de Physique*, vol. lviii. p. 5. *Répertoire de Chimie*, vol. ii. p. 167.

less, mobile, etherial liquid: it is *distanethyle*, discovered by Frankland and Buckton. It is not a radical, for it does not unite directly either with oxygen, chlorine, or iodine. Cahours describes a number of the salts of the *oxide of stanethyle*, which is obtained by decomposing iodide of stanethyle by ammonia. They are mostly obtained by directly combining the base and acid. The salts are decomposed by the action of heat.

The *iodide of sesquistanethyle*, $\text{Sn}^2 (\text{C}^4 \text{H}^5)^3 \text{I}$, is a heavy oil which boils between 235° and 238° . When treated by iodine, it gives iodide of ethyle and iodide of stanethyle.

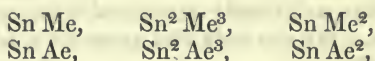
Oxide of sesquistanethyle is obtained in the state of hydrate, $(\text{C}^4 \text{H}^5)^3 \text{Sn}^2 \text{O}$, HO , by treating the iodide with an aqueous solution of potash and distilling: it crystallizes in colourless prisms, which melt between 44° and 45° .

Its aqueous solution acts upon colouring matters like a strong base. Its salts are soluble, crystallizable, and possess a strong odour; they are obtained by treating the oxide with strong acids.

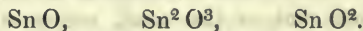
The iodide of methyle, when treated either with pure tin, or an alloy of sodium and tin, yields similar results to those of iodide of ethyle. Cahours has, however, not been able to isolate stanmethyle and distanmethyle in a state of purity.

The salts of stanmethyle and of sesquistanmethyle are obtained by directly combining the oxides with acids. They crystallize extremely well. The salts of sesquistanmethyle are isomorphous with those of sesquistanethyle.

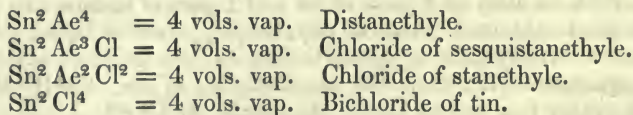
Cahours concludes from these researches that tin forms three definite compounds with methyle, which may be written in the following manner:—



which correspond to the oxides of tin,



If the vapour-densities are taken into account, the formula of some of them must be doubled, and they may then be compared to the formula of bichloride of tin. Thus:



The relations between all these compounds are seen in the action of iodine on distanethyle. Iodide of ethyle is separated, and according to the proportions of iodine taken, equivalents of ethyle are successively removed and replaced by iodine, until biniodide of tin is ultimately obtained.

Magnesethyle.—Magnesium filings are placed in a long glass tube with iodide of ethyle. The reaction set up is moderated by cold water, the tube then sealed before the blowpipe, and heated for some time to 120° or 130° .

The crude product in the tube is distilled in an inert gas; and after suitable rectification, a colourless liquid with an alliaceous odour is obtained, which takes fire in the air, and decomposes water with violence: at the same time the hydrocarbon C^8H^{10} is obtained. It decomposes into C^4H^4 and C^4H^6 .

The action of magnesium on iodide of ethyle is therefore quite similar to that of zinc.

Aluminethyle.—Aluminium, which in the cold is without action on iodide of ethyle, attacks it at 100° , but the reaction is not complete until 130° . A viscous liquid is obtained, which, distilled in hydrogen, is colourless, and possesses an odour like turpentine; it fumes in the air, and decomposes water with violence, producing alumina, hydriodic acid, and a gas which burns with a pale blue flame. It boils between 340° and $350^{\circ}C$.

The formula of this body is $C^{12}H^{15}Al^4I^3 = Al^2I^3, Al^2(C^4H^5)^3$. It takes fire spontaneously in oxygen and in chlorine. It is energetically attacked by zincethyle, yielding an inflammable liquid which is probably *aluminethyle*.

Iodide of methyle attacks aluminium in a similar manner. Glucinum appears to have the same comportment.

Bauer has obtained* a new isomer of aldehyde. He found that chloride of zinc acted with considerable energy on glycol, producing aldehyde and several other oleaginous etherial bodies. When the crude product of the action was treated with chloride of calcium, an insoluble etherial layer rose to the surface, which, when dried and rectified, was found to boil at 110° . It is polymeric with aldehyde and oxide of ethylene: its vapour-density, 2.877, corresponds to the formula $C^4H^8O^2$.

It appears to be a derivative of aldehyde; for, when heated with acetic acid, it does not form acetate of glycol; and it is also produced by the action of chloride of zinc on aldehyde. Bauer names it *acre-aldehyde*. It mixes with alcohol in all proportions; it reduces ammoniacal nitrate of silver; it has a very acrid taste, and a penetrating smell.

Kündig has examined† the action of chlorine on valerianic aldehyde. When a current of chlorine is passed into valeral, the temperature rises; to terminate the reaction, however, external heat must be applied. The liquid product, on being distilled,

* *Comptes Rendus*, July 9, 1860.

† *Liebig's Annalen*, April 1860.

boils at 100° , disengaging hydrochloric acid gas, and the temperature gradually rises to 190° , when nothing but a black residue is left. The distillate between 140° and 148° consists mainly of a chlorinated derivative of valerianic aldehyde, $C^5H^8Cl^2O$. It forms a crystallized compound with bisulphite of soda. No chloride of valeryle, C^5H^9ClO , is produced in the reaction.

When chlorine acts upon valeral in the sunlight, the result is the same; but if the chlorine is in excess, the crystallized compound with bisulphite of soda is not obtained.

Cahours obtained a body, chloramylal, by the action of chlorine upon amylic alcohol, for which Gerhardt has proposed the formula $C^5H^8Cl^2O$. Kündig repeated Cahours' experiments, and obtained a compound similar to that from valeral.

Hübner and Geuther have published* some investigations on acroleine. For the preparation of acroleine, 200 grms. of glycerine were treated in a retort of 20 cubic centimetres capacity with 400 grms. of bisulphate of potash. The product amounted to from 25 to 28 grms. of pure acroleine.

When pentachloride of phosphorus is mixed with acroleine, a brisk action is set up unattended by the disengagement of gas. The parts boiling below 100° are mixed with water, which precipitates a colourless ethereal oil resembling chloroform. When pure it boils at 84° , and its density is 1.170. Its formula is $C^6H^4Cl^2$, and the authors name it *chloride of acroleine*†. It gives, with ammonia, sal-ammoniac and acroleine-ammonia. It is transformed by chlorine into a crystalline body, probably sesquichloride of carbon, C^4Cl^6 . Heated with strong alcoholic potash in a closed vessel, it yields a few drops of a volatile liquid which appeared to be C^6H^3Cl .

Acroleine and ammonia combine to form a white amorphous body, which resembles coagulated albumen. It is obtained by dissolving acroleine in a small quantity of alcohol, and gradually adding an alcoholic solution of ammonia. When dried over sulphuric acid it becomes reddish-brown coloured, transparent, and very hard. It dissolves easily in acids, and is precipitated by caustic or carbonated alkalies. It forms double salts with bichloride of platinum and with bichloride of mercury.

Analysis gave for it the formula $C^{12}H^{10}NO^3$, or $C^{12}H^9NO^2, HO$.

Acroleine unites with *anhydrous acetic acid* to form a compound analogous to that which Geuther obtained by heating aldehyde

* Liebig's *Annalen*, April 1860.

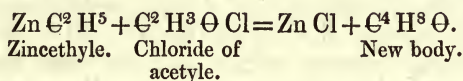
† Wurtz (*Répertoire de Chimie*, June 1860) objects to this name as leading to a false idea of its constitution: it is not a combination of chlorine and acroleine, as this name would imply. He suggests the names chloride of chlorallyle, or chloride of allydene, meaning by allydene the group C^6H^4 .

with anhydrous acetic acid. It boils at 180° , and has the formula $C^{14}H^{10}O^8 = C^6H^4O^2, 2C^4H^3O^3$. It is decomposed by potash into acroleine and acetate of potash.

When acroleine is treated with bisulphite of soda, a solution is obtained which does not possess the odour of acroleine. It does not disengage acroleine when treated with carbonate of soda, nor sulphurous acid when treated with sulphuric acid.

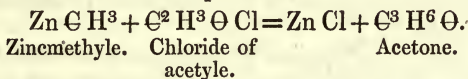
Freund has made a series of experiments* on the synthesis of acetone. By the action of chloride of acetylene on zinc ethyle, he obtained a liquid difficultly soluble in water, and with an odour resembling that of acetone. The boiling-point was found to be between $77^{\circ}\cdot 5$ and $80^{\circ}\cdot 5$. When agitated with a concentrated solution of bisulphite of soda, it became warm and gave a crystalline compound.

The formula of the body is $\text{C}^4\text{H}^8\text{O}$, and its formation may be thus expressed:—



Chloride of propionyle and zincethyle gave a body of the composition $C^5 H^{10} O$, difficultly soluble in water, with the odour of acetone. Its boiling-point was between 100° and 101° C.

The action of chloride of acetylene on zincmethyl gave rise to a copious disengagement of gaseous products; a liquid was obtained which was entirely soluble in water, and also a liquid insoluble in water, which is now under investigation. From the part which dissolved in water a liquid was separated, which boiled between 56° and 60° C. It had an odour exactly resembling that of acetone, and gave with bisulphite of soda a compound crystallizing in nacreous laminae. It was found to have the formula C^3H^6O , and is consequently identical with acetone. Its formation would be thus expressed:—



Fittig has continued† the investigation of *pinakone*‡, the body obtained by the action of sodium on acetone. It dissolves in sulphuric acid to a clear colourless liquid, which when heated became turbid, and separated as a yellowish oil, which distils over colourless with aqueous vapour. This oil has the composition $C^{12}H^{12}O^2$. It is also produced by the action of hydrochloric acid and of chlorine on pinakone; in the latter case it is accompanied by small quantities of substitution products. Fittig

* Liebig's *Annalen*, July 1860.

† Ibid. April 1860.

‡ Phil. Mag. vol. xix. p. 117.

names the oil *pinakoline*. It is a colourless transparent liquid, with a pleasant peppermint odour. Its specific gravity is 0.8, and it boils at 105° . From pinakoline the crystals of pinakone could not be reproduced; and Fittig thinks that it is not identical with anhydrous pinakone, but is isomeric or polymeric with it.

By the action of chlorine on pinakoline, *bichloropinakoline*, $C^{12}H^{10}Cl^2O^2$, is formed. It crystallizes in fine needles, has a very intense odour, and attacks the eyes violently. It boils at 178° . It dissolves in alcohol, from which it is precipitated by water. It is not attacked by concentrated caustic potash. It does not combine with alkaline bisulphites.

MM. Loir and Drion have described* a method by which many of the gases may be liquefied in considerable quantities. It depends on the cold produced by the evaporation of volatile liquids, which was first used by M. Bussy in the liquefaction of ammoniacal gas.

In describing the liquefaction of a gas, authors have generally contented themselves with saying that it could be effected by a certain freezing mixture, which in many cases has a lower temperature than is absolutely necessary. Hence the liquefaction of gases is generally thought to be a more difficult operation than really is the case.

By blowing a dried current of air, by means of a blowpipe bellows, through several tubes into about 7 ounces of ether, a temperature of $-34^{\circ}C$. can be obtained: this temperature, which is reached in about four to five minutes, and can be kept pretty constant for fifteen to twenty minutes, is more than sufficient to liquefy a considerable quantity of *cyanogen gas*. By regulating the rapidity of the air-current, it was found that the temperature of liquefaction is -22° . By blowing slightly through an ordinary pair of bellows over the surface of the liquid gas it solidifies immediately.

By a similar arrangement a large quantity of *sulphurous acid* may be liquefied.

Chlorine cannot be liquefied by means of ether cooled to $-34^{\circ}C$.; but when liquid sulphurous acid is substituted for ether in the foregoing experiment, considerable quantities of liquid chlorine may be obtained.

Ammonia may also be obtained in the liquid state by means of cooled sulphurous acid; the minimum temperature of which is -50° , while liquid ammonia boils at $-35^{\circ}7$.

When liquid ammonia is used as a cooling agent, by rapidly evaporating it under the air-pump in the presence of sulphuric acid, a temperature of $-87^{\circ}C$. is attained; the limit of the lowering of the thermometer is determined by the total solidi-

* *Bulletin de la Société Chimique*, p. 184.

fication of the ammonia. By this temperature Loir and Drion are able to liquefy carbonic acid under the atmospheric pressure. They have also prepared liquid carbonic acid by heating bicarbonate of soda placed in one of the branches of a sealed tube. On cooling, the carbonate of soda reabsorbs the carbonic acid gas.

The authors intend to investigate the physical and also chemical properties of the liquid gases prepared at these low temperatures; under these conditions the ordinary affinities are greatly modified. For instance, 20 cubic centimetres of liquid ammonia placed on a quantity of concentrated sulphuric acid, showed no action at first. Gradually an action was set up and the liquids combined, but with much less violence than might be expected.

The temperatures were measured in these observations by means of an absolute alcohol thermometer, the fixed points of which were determined by means of the temperature of melting ice, and of that of about 2 pounds of frozen mercury. The temperature of the latter was assumed to be -40° C.

XXVII. *On Poncelet's approximate linear Valuation of Surd Forms.* By J. J. SYLVESTER, A.M., F.R.S., Professor of Mathematics at the Royal Military Academy, Woolwich*.

M. PONCELET'S method of approximately representing surd forms, and more particularly the square roots of homogeneous quadratic functions by linear functions of the variables, is given in Crelle's *Journal*, vol. xiii. 1834, pp. 277-291, under the title "Sur la Valeur approchée des radicaux." By this method, as applied to two variables, the resultant of two forces in a plane may be approximately expressed as a linear function of its two components, a case fully considered by M. Poncelet; and tables have been worked out applicable to this case, which appear to have been found of great utility in some important problems of mechanical and practical engineering. But the illustrious author of this beautiful method has left his theory imperfect in respect of its application to three variables.

To supply this slight but not unimportant omission, and to indicate how this more general case admits of being treated, more especially with reference to the approximate representation of the resultant of three forces in space as a linear function of its three components, is the object of this communication. At the close of the memoir referred to, M. Poncelet uses these words:—"Il serait inutile de pousser plus loin cet examen (referring to a dis-

* Communicated by the Author, having been read at the Mathematical Section of the British Association, June 1860.

cussion of the form $\sqrt{a^2 - b^2}$, attendu que dans les applications de la mécanique aux machines les radicaux de la forme $\sqrt{a^2 - b^2}$ sont rarement à considérer. Nous en dirons autant de ceux de la forme $\sqrt{a^2 + b^2 + c^2}$, qui représentent la résultante de trois forces rectangulaires entre elles et situées dans l'espace. D'ailleurs, si l'on connaît les limites entre lesquelles demeurent compris les rapports des composantes a, b, c , ou de leurs résultantes partielles $\sqrt{a^2 + b^2}$, &c., on pourra toujours ramener ce cas au premier de ceux que nous avons examinés," meaning to the case of $\sqrt{a^2 + b^2}$. Now, in the first place, it is not clear how this reduction can be effected in general, or indeed in the vast majority of cases that might be proposed. For instance, if we have given $a < \sqrt{b^2 + c^2}$, $a > b$, $a > c$, I do not see how after, according to M. Poncelet's process, $\sqrt{a^2 + b^2 + c^2}$ is put under the form $\alpha a + \beta \sqrt{b^2 + c^2}$ by aid of the limit $a < \sqrt{b^2 + c^2}$, any use can be made of the other limits $a > b$, $a > c$ in further reducing this to the ultimate form $\alpha a + \alpha' \beta b + \beta' \beta c$. Or if we take the still simpler case, where a, b, c are left unlimited, in whatever way we attempt to proceed we shall obtain different approximations, according to the order in which we effect the successive reductions.

Furthermore, in those few exceptional cases where the process indicated by M. Poncelet leads to the use of all the limits given, the form arrived at is not and never can be the true *best* form, defined as such, according to M. Poncelet's own principles, as that which within the given limits has its *maximum* proportional error the least possible. Thus M. Poncelet indicates as the linear form for $\sqrt{a^2 + b^2 + c^2}$, when the given limits are $a^2 > b^2 + c^2$, $b^2 > c^2$, $\cdot 96046a + \cdot 38201b + \cdot 15827c$, with a maximum error textually quoted from his memoir, $\cdot 0507$. It will be seen hereafter that the true best linear form gives a maximum error about one-tenth less than this. But it would be quite easy to give examples in which the maximum error by Poncelet's process should exceed in an indefinite proportion the necessary maximum error. This, for instance, would be the case if we imposed the limitations

$$x^2 + y^2 > \lambda z^2, \quad y^2 + z^2 > \lambda x^2, \quad z^2 + x^2 > \lambda y^2,$$

on taking λ inferior but indefinitely near to 2.

The geometrical method of demonstration given by M. Poncelet for the case of two variables, labours under the inconvenience of *beginning* with a figure of three dimensions, and consequently does not admit of being carried beyond that case, although the result for three variables geometrically stated, when the conditions of the question are set under an appropriate form, are precisely analogous to that obtained by M. Poncelet for two

variables; for whilst his construction is begun in space, his result subsides to a representation *in plano*. But between these two cases there is a very marked distinction; which is, that whilst for a surd radical with two variables every change in the limits proposed gives rise to a change in the corresponding linear form, such is never the case with a surd form with three or more variables, unless the limits be expressed by a *single linear* inequality between the variables which enter into the surd form, and the surd form itself. Thus, for instance, if $\sqrt{x^2 + y^2 + z^2}$ is to be represented linearly within the limits $z > x, z > y$ (for greater conciseness I throughout suppose the variables to be positive), the linear representation will be precisely the same as for the single limit $z > \sqrt{x^2 + y^2}$, or, which is the same thing, $z - \sqrt{\frac{1}{2} \sqrt{x^2 + y^2 + z^2}} > 0$; and accordingly for the problem with three variables there is usually a preliminary question to be solved, viz. to find the single inequality of the kind proposed which involves the satisfaction of the given limits, and is capable of being substituted for them without increasing the maximum proportional error. This preliminary question may be reduced, as will be seen, to an elementary geometrical form, and is strictly tantamount to the problem following:—Imagine a pincushion with a number of pins stuck into it, to find the least ring which can be made to take them all in,—a problem proposed by myself some four or five years ago with reference to points in a plane, in the Quarterly Mathematical Journal, and of which Professor Peirce of Cambridge University, U.S., has favoured me with a complete solution, which is equally applicable to the sphere, the case with which we shall be principally concerned in what follows.

I shall begin, then, with supposing R to be an integer homogeneous quadratic function of x, y, z , where x, y, z, R are subject to the linear inequality $Ax + By + Cz - \sqrt{R} > 0$. The geometrical solution, as such, will be seen to be equally applicable to the case of two, and the analytical representation to which it leads to any number of variables.

The problem to be solved is to find a linear form $Lx + My + Nz$ such that the greatest value of $\frac{(Lx + My + Nz)}{\sqrt{R}} - 1$ shall have

the least possible arithmetical magnitude, without regard to *sign* as positive or negative, for all values of x, y, z satisfying the proposed inequality.

It is clear that, as the entire question is one of ratios, we may subject x, y, z to the condition expressed by $R=1$ without affecting the result; in other words, we may consider x, y, z as the coordinates of a point limited to lie on the segment of the

surface $R=1$ cut off by the plane $Ax+By+Cz=1$. Suppose, then, that $Lx+My+Nz$ is the linear form sought. The proportional error is $Lx+My+Nz-1$; so that if we draw the plane $Lx+My+Nz-1=0$, the error is expressible geometrically (paying no attention to sign) as the quotient of the perpendicular upon this plane from any point x, y, z in the segment, viz. $\frac{Lx+My+Nz-1}{\sqrt{L^2+M^2+N^2}}$, divided by the perpendicular from the origin in the same plane, viz. $\frac{1}{\sqrt{L^2+M^2+N^2}}$. Hence, then,

the geometrical question to be resolved is simply to draw a plane for which the greatest value of this quotient, restricted to points within the segment, shall be the least possible. From this it is immediately seen to follow, that the portion of the surface cut off by the plane $Lx+My+Nz-1=0$ must be a portion of the segment cut off by the given plane $Ax+By+Cz-1=0$. And its actual position may be determined by means of a principle generally known, but which, as it will occupy but a few words, it may be well to deduce from first principles.

Suppose there are $(r+1)$ quantities, each containing the same system of r parameters; for greater brevity, say three quantities, p, q, r , each functions of the same two parameters λ, μ : let us call the greatest of the quantities p, q, r , corresponding to assigned values of λ, μ , the *dominant*; so that, according as we change λ, μ , the name of the dominant is liable to change; and that we wish to find M the minimum value of the dominant upon the supposition that the variations of p, q, r in respect to λ or μ are never simultaneously zero, and may be made positive or negative at will; then M will be found from the equations $M=p=q=r$. For if we had $M=p$ and $p > q, p > r$, by varying at will λ or μ we could make δp negative; and consequently since by hypothesis p differs sensibly from q and r , the dominant of $p+\delta p, q+\delta p, r+\delta r$ would necessarily be less than that of p, q, r , and thus M would not be the maximum dominant.

In like manner, if $M=p=q, p > r$, we could by means of the equations

$$\begin{aligned}\frac{dp}{d\lambda} \delta\lambda + \frac{dp}{d\mu} \delta\mu &= -\epsilon, \\ \frac{dq}{d\lambda} \delta\lambda + \frac{dq}{d\mu} \delta\mu &= -\eta\end{aligned}$$

so determine $\delta\lambda, \delta\mu$ as to diminish simultaneously p and q ; and thus the dominant of $p-\epsilon, q-\eta, r+\delta r$ would, as before, be less than that of p, q, r . The same reasoning applies to any number $(r+1)$ functions of r variables. And if the number of functions should exceed $r+1$, it would still serve to show that when

the dominant is a maximum, $(r+1)$ out of the whole number of the functions must all alike represent that dominant. Thus leaving for a moment in our original problem the case of three variables, and going down to that of only two variables, in which case we have to deal with a curve of the second order in lieu of a surface, and are to suppose that a segment of such curve is cut off by a right line A, and are required to draw another right line B such that the maximum square of the quotient of a perpendicular upon B from any point in the segment by the perpendicular from the centre upon B is to be a minimum, we evidently have to solve the same problem as if we had to find the least value of the dominant of three quantities involving two parameters, two being the number of constants required to fix the line B; those three quantities being the squares of the fractions whose numerators are the three perpendiculars from the extremities of A, and from the vertex of the arc cut off by B upon B, and their denominators the perpendicular upon B from the origin; accordingly the line B must be so chosen as to make the three perpendiculars in the numerators, without reference to sign, all equal, so that B is parallel to A, and bisects the sagitta of the segment cut off by A, *i. e.* the longest perpendicular from any point in the segment upon A.

In the case of R being, as originally supposed, a function of x, y, z , we may take an indefinite number of points in the section of the surface $R=1$ made by the plane $Ax+By+Cz-1=0$, and the summit of the segment made by the plane to be determined $Lx+My+Nz=1$, and may show by the same reasoning as above (there being now three parameters) that four of these perpendiculars must be equal *inter se*, which proves, to begin with, that at all events the two planes must be parallel; and then the reasoning applied to two functions of one parameter will further show that this plane must bisect the sagitta of the segment cut off by the *given plane* $Ax+By+Cz-1=0$ *. And

* The absolute liberty of the plane sought for ($Lx+My+Nz=1$) to take up all positions in space, and the absence of singular points in the segment cut off by the plane $Ax+By+Cz=1$, suffice to show that the conditions of variation necessary for the legitimate application of the theorem employed above are satisfied. If the minimum dominant is not at one of the *points of equality* given by the theorem, it must lie either at some minimum, or at all events at some singular point of one of the functions of the system to which the dominant belongs, or else at some point corresponding to the contour, so to say, if there be one, of the space within which the parameters are contained. In the case before us, the parameters, however chosen, to fix the position of the plane are perfectly independent, so that there is no limiting contour; and it is obvious that the functions representing the distances concerned from this variable plane have no maxima or minima values. I do not (nor ought I to) pretend to have presented the theoretical principles involved in the limitation of the general

we have now a geometrical solution of the question, which it is important to observe is in general, but, as will be presently seen, not universally applicable to the case when the limiting relations of x, y, z are defined by means of the position of a variable point limited to lie within a triangular area upon the surface $R=1$, whose sides are determined by the traces upon that surface or three planes drawn through the origin; the plane drawn through the angular points of this triangle will then take the place of the plane $Ax + By + Cz - 1 = 0$ in the preceding investigation.

The next thing to be done is to obtain the quantities L, M, N in terms of A, B, C , and the coefficients of R , which is an easy matter to accomplish. Let

$$R = ax^2 + by^2 + cz^2 + 2fyz + 2gzx + 2hxy = \phi(x, y, z),$$

and call ξ, η, ζ the coordinates at the summit of the segment; the equation to the tangent plane at that point, which is of the form $Ax + By + Cz = 0$, will be identical with

$$(a\xi + h\eta + g\zeta)X + (h\xi + b\eta + f\zeta)Y + (g\xi + f\eta + c\zeta)Z = 1.$$

Hence
$$a\xi + h\eta + g\zeta = \frac{A}{\sigma},$$

$$h\xi + b\eta + f\zeta = \frac{B}{\sigma},$$

$$g\xi + f\eta + c\zeta = \frac{C}{\sigma},$$

and

$$\frac{A}{\sigma}\xi + \frac{B}{\sigma}\eta + \frac{C}{\sigma}\zeta = 1;$$

and therefore

$$\frac{1}{\sigma^2} \frac{P\phi(A, B, C)}{\Delta\phi(A, B, C)} = 1,$$

where $\Delta\phi$ is the *discriminant*, and $P(\phi)$ the *polar reciprocal* of $\phi(A, B, C)$. Hence

$$\sigma = \sqrt{\frac{P^*}{\Delta}},$$

law of equality with all the logical rigour and precision of which the subject might admit, as this would be beside my present object, which is not to call in question the grounds of admitted truth applicable to the question in hand, but to advance it one step further in the direction of practical application.

* We see from the above, that if $Ax + By = 1$, or $Ax + By + Cz = 1$ be the equation to the chordal line or plane of a segment of a line or surface of the second degree, the ratio of the perpendiculars to such line or plane from the centre of the line or surface and the vertex of the segment respectively, or, which is the same thing, of a ray to any point in the segment to the portion of this ray produced, intercepted between the line or surface and the tangent at the vertex, is expressed by $\sqrt{\Delta} : \sqrt{P}$. It may at

and the perpendicular upon the tangent plane is

$$\frac{1}{\sqrt{A^2+B^2+C^2}} \sqrt{\frac{P}{\Delta}}.$$

Consequently the mean between this and the perpendicular upon the given plane is

$$\frac{1}{\sqrt{A^2+B^2+C^2}} \frac{\sqrt{P} + \sqrt{\Delta}}{2\sqrt{\Delta}};$$

and therefore the equation to the plane required is

$$Ax + By + Cz = \frac{\sqrt{P} + \sqrt{\Delta}}{2\sqrt{\Delta}},$$

so that

$$L = \frac{2\sqrt{\Delta}}{\sqrt{P} + \sqrt{\Delta}} A, \quad M = \frac{2\sqrt{\Delta}}{\sqrt{P} + \sqrt{\Delta}} B, \quad N = \frac{2\sqrt{\Delta}}{\sqrt{P} + \sqrt{\Delta}} C,$$

$Lx + My + Nz$ being the approximate representation of $\sqrt{\phi(x, y, z)}$, and the maximum error being evidently

$$\frac{\sqrt{P} - \sqrt{\Delta}}{\sqrt{P} + \sqrt{\Delta}}.$$

These results are perfectly general, and apply to a quadratic radical of an integer homogeneous quadratic function of any number of variables; thus for $\sqrt{\phi(x, y, z, t)}$ the linear representative form is

$$\frac{2\sqrt{\Delta}.A}{\sqrt{P} + \sqrt{\Delta}} x + \frac{2\sqrt{\Delta}.B}{\sqrt{P} + \sqrt{\Delta}} y + \frac{2\sqrt{\Delta}.C}{\sqrt{P} + \sqrt{\Delta}} z + \frac{2\sqrt{\Delta}.D}{\sqrt{P} + \sqrt{\Delta}} t,$$

and the greatest proportional error is still

$$\frac{\sqrt{P} - \sqrt{\Delta}}{\sqrt{P} + \sqrt{\Delta}};$$

D signifying the discriminant, and P the polar reciprocal of $\phi(A, B, C, D)$.

For the sphere, the perpendicular upon any tangent plane being 1, the linear form ought to be that obtained from the equation $Ax + By + Cz = K$, where

$$\frac{K}{\sqrt{A^2+B^2+C^2}} = \frac{1}{2} \left(1 + \frac{1}{\sqrt{A^2+B^2+C^2}} \right),$$

first sight appear strange that P should be of the form of a *contravariant* (in lieu of a *covariant*); but it must be remembered that the axes to which the line or surface and its chord are referred are supposed to be orthogonal, and for orthogonal substitutions, contravariants and covariants are indistinguishable.

or

$$K = \frac{1}{2}(\sqrt{A^2 + B^2 + C^2} + 1),$$

that is to say, the approximation is

$$\frac{2A}{1 + \sqrt{A^2 + B^2 + C^2}}x + \&c.,$$

the maximum error being

$$\frac{\sqrt{A^2 + B^2 + C^2} - 1}{\sqrt{A^2 + B^2 + C^2} + 1},$$

which is easily seen to agree with the general formulæ above given.

When, as is usually the case in applying these results, the plane $\Lambda x + By + Cz - 1 = 0$ is not directly given, but is to be found as the plane passing through three given points whose coordinates are a, b, c ; a', b', c' ; a'', b'', c'' respectively, we may use the equations

$$A = \frac{F}{Q}, \quad B = \frac{G}{Q}, \quad C = \frac{H}{Q},$$

where

$$F = (b'c'' - b''c') + (b''c - bc'') + (bc' - b'c),$$

$$G = (c'a'' - c''a') + (c''a - ca'') + (ca' - c'a),$$

$$H = (a'b'' - a''b') + (a''b - ab'') + (ab' - a'b),$$

$$Q = \begin{vmatrix} a & b & c \\ a' & b' & c' \\ a'' & b'' & c'' \end{vmatrix}$$

But it may also sometimes be needful in practice, as will presently appear, to determine the plane with immediate reference to only two points upon the surface.

Application to the surd form which represents the resultant of three forces at right angles to each other.

Here $R = \sqrt{x^2 + y^2 + z^2}$, and $R = 1$ represents a sphere. Two cases will be shown to arise. The first, the more frequent one, is that already alluded to, where a limiting plane has to be drawn through three given points. For this case, using F, G, H in the sense in which they have immediately above been employed, the linear representation of $\sqrt{x^2 + y^2 + z^2}$ becomes

$$\frac{2F}{Q+N}x + \frac{2G}{Q+N}y + \frac{2H}{Q+N}z,$$

with a maximum proportional error

$$\frac{N-Q}{N+Q'}$$

N representing

$$\sqrt{F^2 + G^2 + H^2}.$$

The second case is where the limiting plane has to be drawn through two points upon the sphere so as to cut it in a circle, of which the line joining the two points is a diameter.

In this case, calling the coordinates of the two points respectively $\alpha, \beta, \gamma; \alpha', \beta', \gamma'$, and writing $\alpha\alpha' + \beta\beta' + \gamma\gamma' = m$, it is easily seen that the perpendicular upon the limiting plane is

$\sqrt{\frac{1+m}{2}}$, and consequently the perpendicular upon the plane

$$Lx + My + Nz = 1 \text{ is } \frac{1}{2} \left\{ 1 + \sqrt{\frac{1+m}{2}} \right\}.$$

Also this plane being parallel to the limiting plane, is perpendicular to the line joining the origin to the point

$$x : y : z :: \frac{\alpha + \alpha'}{2} : \frac{\beta + \beta'}{2} : \frac{\gamma + \gamma'}{2},$$

and therefore

$$L = \frac{(\alpha + \alpha')}{\rho}, \quad M = \frac{(\beta + \beta')}{\rho}, \quad N = \frac{(\gamma + \gamma')}{\rho},$$

and

$$\frac{\rho}{\sqrt{(\alpha + \alpha')^2 + (\beta + \beta')^2 + (\gamma + \gamma')^2}} = \frac{1}{2} \left\{ 1 + \sqrt{\frac{1+m}{2}} \right\};$$

that is to say,

$$\begin{aligned} \rho &= \sqrt{2(1+m)} \cdot \frac{1}{2} \left(1 + \sqrt{\frac{1+m}{2}} \right) \\ &= \frac{1}{2} \{ \sqrt{2(1+m)} + (1+m) \}; \end{aligned}$$

so that the linear form required is

$$\left\{ \sqrt{2(1+m)} + 1 + m \right\} \left\{ \frac{\alpha + \alpha'}{2} x + \frac{\beta + \beta'}{2} y + \frac{\gamma + \gamma'}{2} z \right\},$$

with a maximum proportional error

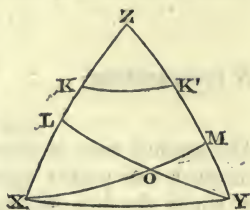
$$\frac{\sqrt{2} - \sqrt{1+m}}{\sqrt{2} + \sqrt{1+m}}.$$

(m is of course identical with the cosine of the angle between the radii joining the two given points.)

The conditions of inequality which obtain between x, y, z may be, and usually will be, such as correspond to the limitation of the point (x, y, z) to an area contained within a triangle or

polygon upon the surface of the sphere. Thus take X, Y, Z, each a quadrant apart from the other, the points where the surface of the sphere $x^2 + y^2 + z^2 = 1$ is pierced by the axis. If no limitation is placed upon the values of x, y, z further than the one throughout supposed of their remaining always positive, the limiting area will be XYZ. If we suppose

$$Z > k\sqrt{x^2 + y^2},$$

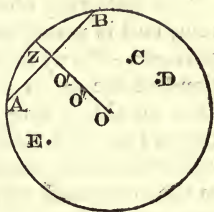


we may take $\tan XK = k$, and drawing the small circle KK' , ZLM will be the limiting area; if, again, $Z < k\sqrt{x^2 + y^2}$, $KK'YX$ will be the limiting area; if, again, $Z < k\sqrt{x^2 + y^2}$, $Z > lx$, $Z > my$ be the limiting conditions, taking $\tan LX = l$, $\tan MY = m$, and drawing LY , XM to intersect in O , $KK'MOL$ will be the corresponding area, and so in general. Even so simple a set of conditions as $Z > x$, $Z > y$ it is seen will give rise to a quadrilateral area, limited in the figure by $ZLOM$, when $ZL = ZM = 45^\circ$. Thus, then, we approach the preliminary question to which allusion has been already made, which is to determine the *least circle* that will cut off from a given sphere a segment containing all a given system of points lying upon it. The solution is precisely the same, substituting arcs of great circles for right lines, as the problem of drawing upon a plane the least circle containing a set of points given in the plane.

We may, in the first place, obviously reject all those points that are contained within the contour formed by arcs joining the remaining points, so that the case of points lying at the angles of a convex polygon alone remains to be studied. Now if we confine our attention even to the simplest case of a system of three points, we shall see at once that two cases arise. If a circle be drawn through them, and these three points do not lie in the same semicircle, no smaller circle than this can be drawn to contain the three; but if they do lie in the same semicircle, it is obvious that a circle described upon the line joining the outer two as a diameter will be smaller than the circle passing through all three, and will contain them all. It was this simple but striking fact in the geometry of situation which led me to propose the question for any number of points in the *Quarterly Mathematical Journal*; and as Prof. Peirce's exhaustive method of solution has not appeared in print, I may take this occasion of presenting it.

Let A, Z, B, C, D, E be the given points. Let AZB be a circle whose centre is drawn through A, Z, B , chosen so as to include all the others; then if AZB are not contained in the

same semicircle, $A Z B$ is the circle required. But if $A Z B$ be less than a semicircle, as in the figure, we may first reject the consideration of all the points contained between the arc $A B$ and its chord. We must then find $O', O'', \&c.$, the centres of the circles passing through A, B, C ; A, B, D , &c. : these will all lie in the same



straight line $O'O''O$. Selecting the one the nearest to O , say O'' , we describe the corresponding circle, in which $A C$ will now take the place of $A B$ in the former circle. If the points A, B, C are not contained in less than a semicircle, *i. e.* if $A B C$ is an acute-angled or right-angled triangle, $A B C$ is the circle required; but if they do lie within the same semicircle so that $A B C$ forms an obtuse angle, B will now have to be rejected, and we must find a new centre as before, and so on continually. By this process we must inevitably at last exhaust all the given points; and the final circle so obtained will be the circle sought, unless the three points through which it has been drawn are distributed over the same semicircle, in which case the circle required is that described upon the chord joining the two extreme points as its diameter. The solution will evidently be *unique*, and (as already hinted at) merely require the construction upon the sphere either of a circle passing through a certain set of three out of all the given points, or else passing through only two of them, so as to be perpendicular to the radius bisecting their joining line.

If we imagine an india-rubber band (similar, we may suppose, in form to a "parlour quoit" but more elastic) having the faculty of maintaining its figure always circular, or which is more simple in the case before us, capable of maintaining itself in the same plane, and imagine this sufficiently stretched over the surface of the sphere to contain all the given points (represented by very minute pins' heads given upon it), this band will by its contraction upon the surface of the sphere, however originally placed, imitate the steps of Prof. Peirce's method of solution; and after (it may be) passing through and quitting successive sets of three points, come to a position of *geometrical equilibrium*, either when its circumference contains a triad of the given points lying at the angles of an acute-angled triangle, or a duad at the extremities of one of its diameters*.

* The annexed is a more complete and, I think, a correct account of what would happen to the band under the supposed conditions. It will begin to move parallel to its own plane, and continue so to do until it comes in contact with one of the physical points (call it A) upon the surface of the sphere. Supposing that the position of equilibrium is not then attained

The following observation, which constitutes a veritable theorem, and is presupposed in Prof. Peirce's solution, is very important:—"Any circle being found which, either passing through three of the given points such that no two of their joining lines form an obtuse angle, or which described upon the line joining two of the given points as a diameter, includes all the rest, is the minimum circle which contains all the points of the given cluster; so that one, and only one, circle exists satisfying the above *alternative condition*."

It may be instructive to proceed to the application of the method now fully explained to some of the more salient cases of inequality, it being understood that these cases are given to afford some general notion of the precision of the method, and by no means as specimens of such as it would be applied to in practice, for which the limits I shall suppose would be far too wide to furnish any useful result.

Ex. 1. x, y, z unlimited. Here the values of F, G, H, Q are the minor determinants of the matrix,

$$\begin{vmatrix} 1 & 0 & 0 & \bar{1} \\ 0 & 1 & 0 & \bar{1} \\ 0 & 0 & 1 & \bar{1} \end{vmatrix}$$

$F=G=H=1, Q=1$, and the linear approximation to $\sqrt{x^2+y^2+z^2}$ becomes $\frac{2}{\sqrt{3}+1}x + \&c.$, or $(\sqrt{3}-1)x + (\sqrt{3}-1)y + (\sqrt{3}-1)z$,

by the band passing at the same moment through *one* other point at the opposite extremity of a diameter to A, or through *two* other of the given points forming a non-obtuse-angled triangle with A, it will begin to revolve (always contracting the while) about a tangent at A to its intersection with the sphere as an axis, until it meets a second of the given points, say B. If the line A B is a diameter of the band, *cadit quæstio*, the problem is solved. If not, the band will go on further contracting, revolving meanwhile round A B as an axis until either A B becomes a diameter in virtue of the contraction of the band's dimensions (and so the problem is solved), or else before this can take place the band is arrested at a third point C, either forming a non-obtuse-angled triangle with A B and so solving the problem, or else an obtuse-angled triangle with A B and lying exterior to the arc A B on one side of it or the other; on the latter supposition the line joining C with the extremity of A B nearest to it, will (it appears to me) form a new axis of rotation for the band, which will quit the further extremity of the old axis, and thus the motion will continue with an intermitting change of axes, until at last the band either finds out for itself an axis which in the course of the contraction becomes a diameter, or else brings the band into contact with a third point forming a non-obtuse-angled triangle with such axis, in either of which cases the minimum periphery is attained, the contraction comes to an end, and the problem is solved.

or say

$$\cdot 73025x + \cdot 73025y + \cdot 73025z,$$

with a maximum proportional error

$$\frac{\sqrt{3}-1}{\sqrt{3}+1}, \text{ or } 2-\sqrt{3}=\cdot 26895.$$

The corresponding error for $\sqrt{x^2+y^2}$ under the form $\cdot 8284x + \cdot 8284y$ is $\cdot 17160$, or about two-thirds of the one in question*.

Ex. 2. $z > \sqrt{y^2+x^2}$. Here the determining matrix is

$$\begin{array}{cccc} 0 & 0 & 1 & \bar{1} \\ 0 & \sqrt{\frac{1}{2}} & \sqrt{\frac{1}{2}} & \bar{1} \\ \sqrt{\frac{1}{2}} & 0 & \sqrt{\frac{1}{2}} & \bar{1} \end{array} \quad \begin{array}{l} F=G=\sqrt{\frac{1}{2}}-\frac{1}{2}=\cdot 207107 \\ H=\frac{1}{2} \\ Q=\frac{1}{2} \end{array}$$

$$N^2=F^2+G^2+H^2=1-\sqrt{\frac{1}{2}}=\cdot 292893$$

$$N=\cdot 541196$$

$$N+Q=1\cdot 041196 \quad N-Q=\cdot 041196.$$

Thus the linear approximation becomes

$$\cdot 397825x + \cdot 397825y + \cdot 960430z,$$

with a maximum error $\cdot 039493$.

Ex. 3. $z > \sqrt{y^2+x^2}$, $y > x$. This is M. Poncelet's example (Crelle, vol. xiii. p. 291). His a , b , c correspond respectively with my z , y , x ; there are some misprints in line 6 of this page (in M. Poncelet's Memoir) which may perplex the reader; it is intended to stand thus:

$$\delta \sqrt{a^2+b^2+c^2} + \beta \delta' \sqrt{b^2+c^2} = \sqrt{a^2+b^2+c^2} \cdot \left(\delta + \beta \delta' \sqrt{\frac{b^2+c^2}{a^2+b^2+c^2}} \right).$$

Here the determining matrix corresponds to the area Z K N (the coordinates of N being found from the equations $z^2=x^2+y^2$, $y=x$, $z^2+x^2+y^2=1$), and the matrix will be as subjoined.

* It would have been more exact to have treated this as a case of a circle to be drawn through four points, viz. Z the middle points of ZX, ZY and the middle or lowest point (in reference to Z) of the small circle drawn through these two, and having Z for its pole. But it is easily seen that the small circle drawn through the three former will contain the one last

named, for the tangent of its circular radius will be $\sqrt{2} \times \tan \frac{45}{2}$, and consequently its summit will be further from Z than from the point in question. A similar remark applies to the subsequent and some other examples.

$$\begin{array}{cccc} 0 & 0 & 1 & \bar{1} \\ 0 & \sqrt{\frac{1}{2}} & \sqrt{\frac{1}{2}} & \bar{1} \\ \frac{1}{2} & \frac{1}{2} & \sqrt{\frac{1}{2}} & \bar{1} \end{array}$$

$$F = \sqrt{\frac{1}{2}} + \frac{1}{2}\sqrt{\frac{1}{2}} - \frac{1}{2} - \frac{1}{2} = 3\sqrt{\frac{1}{8}} - 1 = \cdot 060660$$

$$G = \frac{1}{2} - \frac{1}{2}\sqrt{\frac{1}{2}} = \frac{1}{2} - \sqrt{\frac{1}{8}} = \cdot 146447$$

$$H = \frac{1}{2}\sqrt{\frac{1}{2}} = \cdot 353553$$

$$Q = \frac{1}{2}\sqrt{\frac{1}{2}} = \cdot 353553$$

$$\begin{aligned} N^2 &= F^2 + G^2 + H^2 = \frac{17}{8} + \frac{3}{8} + \frac{1}{8} - 7\sqrt{\frac{1}{8}} \\ &= \frac{21}{8} - \frac{1}{2}\sqrt{24\cdot 5} \\ &= 2\cdot 625 - 2\cdot 474874 \\ &= \cdot 150126 \end{aligned}$$

$$N = \cdot 387461, \quad N + Q = \cdot 741014, \quad N - Q = \cdot 033908.$$

The maximum error therefore is $\frac{33908}{741014} = \cdot 0457$, or about one-tenth less than that given by M. Poncelet's form

$$\frac{2F}{N+Q} = \frac{6066}{37051} = \cdot 1637,$$

$$\frac{2G}{N+Q} = \frac{14645}{37051} = \cdot 3953,$$

$$\frac{2H}{N+Q} = \frac{35355}{37051} = \cdot 9542.$$

The last of these quantities is less, the first two greater than the corresponding coefficients in M. Poncelet's form.

Ex. 4 and 5. The inequality system, $\sqrt{x^2 + y^2} > z > y > x$, is represented by the triangle K N Q, and the corresponding determining matrix will be

$$\begin{array}{cccc} 0 & \sqrt{\frac{1}{2}} & \sqrt{\frac{1}{2}} & \bar{1} \\ \frac{1}{2} & \frac{1}{2} & \sqrt{\frac{1}{2}} & \bar{1} \\ \sqrt{\frac{1}{3}} & \sqrt{\frac{1}{3}} & \sqrt{\frac{1}{3}} & \bar{1} \end{array}$$

So, too, the inequality system, $\sqrt{x^2 + y^2} < z < y > x$, has for its locus the triangle Z K N, its determining matrix

$$\begin{array}{cccc} 0 & \sqrt{\frac{1}{2}} & \sqrt{\frac{1}{2}} & \bar{1} \\ \frac{1}{2} & \frac{1}{2} & \sqrt{\frac{1}{2}} & \bar{1} \\ 0 & 0 & 1 & \bar{1} \end{array}$$

It would be superfluous to go on multiplying numerical ex-

amples, that may be left to those who feel the want of the Tables which this method affords. If the limiting conditions were supposed to be $z > y$, $z > x$, this would correspond to the quadrilateral $ZK'QK$ in the last figure: it may easily be ascertained that a circle passing through $K'ZK$ would contain Q , and would have its centre between N and Z . Hence by the application of Peirce's law, we know that the minimum circle in this case is that which can be drawn through $K'ZK$, and consequently the linear form and maximum error will be precisely the same as for the simpler case already considered, $z > \sqrt{x^2 + y^2}$. On the other hand, if the conditions imposed were simply $z < x$, $z < y$ (conditions, be it remembered, far wider than ever would be admitted in practice), the limiting figure becomes XQY ; and since $MQ < MX$ or MY , the centre of the circle through XQY would fall under XY , so that the limiting circle in this case would be that having M for its pole; the linear substitutive form would not contain z , but would be the same as if z did not appear, viz. $\cdot96046x + \cdot960467y$, with $\cdot03954$ as the maximum proportional error. The same remark would apply to the system of conditions $z < \lambda x$, $z < \lambda y$ for any value of λ not inferior to $\sqrt{\frac{1}{2}}$.

The conditions $z > x$, $z > y$, $z < \sqrt{x^2 + y^2}$ would correspond to the limiting area $KK'Q$, which would give rise to the determining matrix,

$$\begin{array}{cccc} 0 & \sqrt{\frac{1}{2}} & \sqrt{\frac{1}{2}} & \bar{1} \\ \sqrt{\frac{1}{2}} & 0 & \sqrt{\frac{1}{2}} & \bar{1} \\ \sqrt{\frac{1}{3}} & \sqrt{\frac{1}{3}} & \sqrt{\frac{1}{3}} & \bar{1} \end{array}$$

The condition $z < \sqrt{x^2 + y^2}$ would correspond to a limiting area, $KK'XY$. If KY be bisected in G , and $K'X$ in G' , and $G'YGX$ intersect in H , it is obvious that a small circle may be described with H as its pole passing through all four points X, Y, K, K' , which will be the minimum circle of limitation. To assign the determining matrix, we may take any three of these four points, as, for example, Y, X, K , which will give

$$\begin{array}{cccc} 0 & 1 & 0 & \bar{1} \\ 1 & 0 & 0 & \bar{1} \\ \sqrt{\frac{1}{2}} & 0 & \sqrt{\frac{1}{2}} & \bar{1} \end{array}$$

This gives

$$\begin{aligned} Q &= \sqrt{\frac{1}{2}} = \cdot70711, \\ F &= \sqrt{\frac{1}{2}}, \quad G = \sqrt{\frac{1}{2}}, \quad H = 1 - \sqrt{\frac{1}{2}} = \cdot29289, \\ N^2 &= \frac{5}{2} - \sqrt{2} = 1\cdot085786, \\ N &= 1\cdot04200, \\ N + Q &= 1\cdot74911, \quad N - Q = \cdot33489. \end{aligned}$$

The linear approximation is accordingly

$$\cdot 8090x + \cdot 8090y + \cdot 3351z,$$

with a maximum proportional error $\cdot 1914$.

Finally, for $z > y$, $y > x$ the limiting triangle will be Z K Q, the determining matrix

$$\begin{array}{cccc} 0 & 0 & 1 & \bar{1} \\ 0 & \sqrt{\frac{1}{2}} & \sqrt{\frac{1}{2}} & \bar{1} \\ \sqrt{\frac{1}{3}} & \sqrt{\frac{1}{3}} & \sqrt{\frac{1}{3}} & \bar{1} \end{array}$$

$$F = \sqrt{\frac{1}{2}} - \sqrt{\frac{1}{3}} = \cdot 1297, \quad G = \sqrt{\frac{1}{3}} \{1 - \sqrt{\frac{1}{2}}\} = \cdot 1692,$$

$$H = \sqrt{\frac{1}{6}} = \cdot 4082,$$

$$N^2 = \frac{3}{2} - \sqrt{\frac{2}{3}} - \sqrt{\frac{2}{9}} = \cdot 21207,$$

$$N = \cdot 4605,$$

$$N + Q = \cdot 8687,$$

$$Q = \sqrt{\frac{1}{6}} = \cdot 4082,$$

$$N - Q = \cdot 0523.$$

The linear approximation is $\cdot 2986x + \cdot 3895y + \cdot 9397z$, with a maximum error $\cdot 06$ (more precisely $\cdot 0602$). This is a trifle beyond half as much again as the maximum error of the best linear approximation to $\sqrt{x^2 + y^2}$, subject to the limitation $x > y$, which (see Poncelet's Memoir, p. 280) is a little under $\cdot 04$.

Poncelet has shown that for $\sqrt{x^2 + y^2}$, when x, y are the co-ordinates of a point limited within a sector whose bounding radii make angles ϕ and ψ with the axis of X, the approximate linear form is

$$\frac{\cos \frac{1}{2}(\phi + \psi)}{\cos^2 \frac{\phi - \psi}{4}} x + \frac{\sin \frac{1}{2}(\phi + \psi)}{\cos^2 \frac{\phi - \psi}{4}} y,$$

with a maximum error $\tan^2 \frac{\phi - \psi}{4}$.

In like manner it follows immediately from the method given in the text, that if the summit of the limiting segment make angles λ, μ, ν with the axes of X, Y, Z, and its spherical radius be ρ , the approximate expression for $\sqrt{x^2 + y^2 + z^2}$ is

$$\frac{\cos \lambda}{\cos^2 \frac{\rho}{2}} x + \frac{\cos \mu}{\cos^2 \frac{\rho}{2}} y + \frac{\cos \nu}{\cos^2 \frac{\rho}{2}} z,$$

with a maximum error $\tan^2 \frac{\rho}{2}$, which expressions are the precise analogues of the former, as will immediately appear from the

consideration that the summit of the spherical segment corresponds with the centre of the circular arc.

As an example of the use of these formulæ, suppose the given limits to be

$$x < \sqrt{y^2 + z^2}, \quad y < \sqrt{z^2 + x^2}, \quad z < \sqrt{x^2 + y^2}.$$

If we bisect the quadrants XY , YZ , ZX in L , M , N respectively, the variable point will be limited to lie in LMN , and the base of the corresponding segment will be the circle passing through LMN whose summit will be at E , the point where the perpendicular to XY at L and the arc bisecting the angle X meet.

Here then we have

$$\rho = LE, \quad \lambda = \mu = \nu = XE,$$

$$\tan \rho = \cos 45 = \sqrt{\frac{1}{2}}, \quad \cot \lambda = \sqrt{\frac{1}{2}},$$

$$\cos \rho = \sqrt{\frac{2}{3}}, \quad \cos^2 \frac{\rho}{2} = \frac{1}{2} \{1 + \sqrt{\frac{2}{3}}\}, \quad \cos \lambda = \sqrt{\frac{1}{3}},$$

$$\tan^2 \frac{\rho}{2} = \frac{\sqrt{3} - \sqrt{2}}{\sqrt{3} + \sqrt{2}}.$$

Hence the linear approximation is

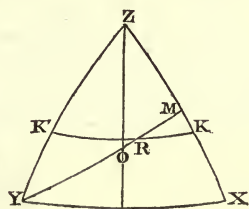
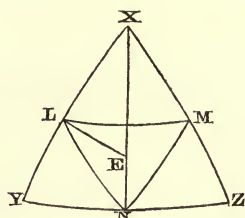
$$\begin{aligned} \frac{2}{\sqrt{3} + \sqrt{2}} (x + y + z) &= 2(\sqrt{3} - \sqrt{2}) \{x + y + z\} \\ &= .6356744(x + y + z), \end{aligned}$$

with a maximum proportional error $5 - \sqrt{24} = .10102$.

More generally, if we assume the system of conditions

$$\sqrt{x^2 + y^2} > cz, \quad \sqrt{y^2 + z^2} > cx, \quad \sqrt{z^2 + x^2} > cy,$$

c being any number intermediate between 1 and $\sqrt{2}$, if in the figure annexed, we take $\tan ZK = \tan ZK' = c$, and join KK' by a small circle intersecting YM which bisects ZX in R , O remaining still the summit of XZY , it is easy to perceive that the limiting area will be included within the triangular space cut out between KK' and the two other analogous small circles; λ , μ , ν will remain the same as before, and OR will represent ρ . Accordingly we have from the qua-



drantal triangle ZYR,

$$\cos ZR = \sin RY \cos RYZ,$$

i. e.

$$\sin RY = \sqrt{\frac{2}{c^2 + 1}};$$

$$\therefore RY = \tan^{-1} \sqrt{\frac{2}{c^2 - 1}}$$

$$\begin{aligned} \tan \rho = \tan RO = \tan RY - \tan OY &= \frac{\sqrt{\frac{2}{c^2 - 1}} - \sqrt{2}}{1 - \frac{2}{\sqrt{c^2 - 1}}} \\ &= \sqrt{2} \left\{ \frac{1 - \sqrt{c^2 - 1}}{\sqrt{c^2 - 1} - 2} \right\}. \end{aligned}$$

When $c = \sqrt{2}$, this vanishes; and when $c > \sqrt{2}$, the conditions become incompatible.

The equations $\tan \phi = \sqrt{\frac{2}{c^2 - 1}}$, or $\cos 2\phi = \frac{c^2 - 3}{c^2 + 1}$, and

$$\rho = \phi - \tan^{-1} \sqrt{2} = \phi - 54^\circ 44'$$

are well adapted for logarithmic computation. Suppose

$$\begin{aligned} c = \frac{4}{3}, \quad \cos 2\phi &= -\frac{1}{2} = -, 44 \quad 2\phi = 180^\circ - 63^\circ 54' = 116^\circ 6', \\ \phi &= 58^\circ 3', \quad \rho = 3^\circ 19', \end{aligned}$$

giving a maximum error $\tan(1^\circ 39' 30'')^2 = .0008375$. The linear form corresponding to this is

$$\frac{2\sqrt{\frac{1}{3}}}{1 + \cos \rho} \{x + y + z\} = .5778x + .5778y + .5778z.$$

If $c < 1$, the formula changes; the limiting area from a triangle becoming a hexagon through all the angles of which a circle will admit of being drawn, which circle will give the limiting segment. ρ becomes the third side of a spherical triangle of which the other two sides are $\tan^{-1} \sqrt{2}$ and $\tan^{-1} c$ respectively, and the included angle 45° ; so that

$$\cos \rho = \sqrt{\frac{1}{3(1+c^2)}} + \sqrt{\frac{c}{3(1+c^2)}} = (1 + \sqrt{c}) \sqrt{\frac{1}{3(1+c^2)}},$$

and the maximum error, *i. e.* $\left(\tan^2 \frac{\rho}{2}\right)$, becomes

$$\frac{\sqrt{3(1+c^2)} - 1 - \sqrt{c}}{\sqrt{3(1+c^2)} + 1 + \sqrt{c}}.$$

The only real difficulty in extending M. Poncelet's method in the manner pursued in the above unpretending study, consisted in forming a clear preconception of the mode in which any given system of limits require for the purpose in view to be regarded, viz. as enveloped, so to say, in a single condition (no wider than absolutely necessary) expressed by a linear equation between the given surd function and the variables which enter into it.

I may in conclusion just observe that if the relative values of the variables be limited, not by a system of conditions giving rise to a polygonal area of limitation, but by a condition expressed by the positivity of a single homogeneous function of the variables of any degree, the variable point will then be limited by the intersection of the sphere with a cone, and we should have to solve a preliminary geometrical problem of circumscribing a spherical curve by the least possible circle,—a question which I have neither leisure nor inclination to discuss, but to which I believe Mr. Cayley has paid some attention.

Before taking final leave of my readers and the subject, I devote a word to the *inverse case* of *Three Rectangular Forces*. This is the case where the resultant and two of the rectangular components are given, and it is the third component which is to be expressed linearly in terms of them. In this case an approximate expression is to be found for $\sqrt{z^2 - y^2 - x^2}$, and the geometrical locus which replaces the sphere becomes an equilateral hyperboloid of revolution of two sheets.

If the variable point be supposed to be limited to a segment of one sheet of the hyperboloid cut off by the plane $Ax + By + Cz = 1$, the discriminant of $z^2 - y^2 - x^2$ being 1, and its polar reciprocal of the same form as itself, the approximate linear form of the surd becomes

$$\frac{2Cz}{\sqrt{C^2 - B^2 - A^2} + 1} + \frac{2By}{\sqrt{C^2 - B^2 - A^2} + 1} + \frac{2Ax}{\sqrt{C^2 - B^2 - A^2} + 1},$$

with a maximum proportional error $\frac{1 - \sqrt{C^2 - B^2 - A^2}}{1 + \sqrt{C^2 - B^2 - A^2}}$.

To *envelope*, however, any given arbitrary system of inequalities between the coordinates x, y, z on the hyperboloid within a single condition, $Ax + By + Cz - 1 > 0$ becomes a geometrical problem of somewhat greater difficulty than the corresponding one for the sphere, and I do not propose to enter upon the discussion of it here.

I shall content myself, as M. Poncelet has done in the corre-

sponding case in *plano*, with exhibiting a single numerical application of the method.

Suppose the given limits to be defined by the equations

$$z^2 > \frac{5}{2} (y^2 + x^2) \quad y > x.$$

Here it is obvious that the *enveloping condition* will be expressible by means of the equation to a plane drawn through three points on the hyperboloid, the coordinates of one of which are found by writing

$$y=0, \quad x=0;$$

of a second by writing

$$z^2 - \frac{5}{2} y = 0, \quad x=0;$$

and of the third by writing

$$z^2 - \frac{5}{2} (y^2 + x^2) = 0, \quad y - x = 0;$$

and for all three

$$z^2 - y^2 - x^2 = 1.$$

Hence we obtain the matrix

$$\begin{array}{cccc} 1 & 0 & 0 & \bar{1} \\ \sqrt{3} & \sqrt{2} & 0 & \bar{1} \\ \sqrt{3} & 1 & 1 & \bar{1} \end{array}$$

And if we call the minors obtained by leaving out the first, second, third, fourth columns respectively H, G, F, Q, the linear form becomes

$$\frac{2Hz}{\sqrt{H^2 - G^2 - F^2} + Q} + \frac{2Gy}{\sqrt{H^2 - G^2 - F^2} + Q} + \frac{2Fx}{\sqrt{H^2 - G^2 - F^2} + Q}$$

with a maximum error $\frac{Q - \sqrt{H^2 - G^2 - F^2}}{Q + \sqrt{H^2 - G^2 - F^2}}$ And since

$$Q = \sqrt{2}, \quad H = \sqrt{2}, \quad -G = \sqrt{3} - 1, \quad -F = (\sqrt{2} - 1)(\sqrt{3} - 1)$$

we have

$$\sqrt{H^2 - G^2 - F^2} = 1.1714 \text{ and } Q = 1.4142,$$

so that the representative form becomes $1.093z - .566y - .089z$, with a maximum relative error of about .094.

XXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

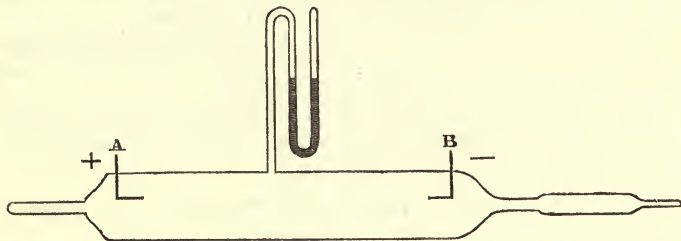
[Continued from p. 164.]

January 19, 1860.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

THE following communication was read:—

“On Vacua as indicated by the Mercurial Siphon-gauge and the Electrical Discharge.” By J. P. Gassiot, Esq., F.R.S.

That the varied condition of the stratified electrical discharge is due to the relative but always imperfect condition of the vacuum through which it is passed, is exemplified by the changes which take place in the form of the striæ while the potash is heated in a carbonic acid vacuum-tube. In order, if possible, to measure the pressure of the vapour, I had a carefully prepared siphon mercurial gauge sealed into a tube *fifteen* inches long, at an equal distance between the two wires A, B.



This tube was charged with carbonic acid in the manner described by me in a former communication. When exhausted by the air-pump and sealed, it showed a pressure indicated by about 0·5 inch difference in the level of the mercury; the potash was then heated; the mercury gradually fell, until it became perfectly level.

Dr. Andrews (Phil. Mag. February 1852) has shown, that with a concentrated solution of caustic potassa, he obtained with carbonic acid a vacuum with the air-pump so perfect as to exercise no appreciable tension, as no difference in the level of the mercury in the siphon-gauge could be detected.

On trying the discharge in the vacuum-tube after the potash had cooled, I found it gave the cloud-like stratifications, with a slight reddish tinge; consequently not only was the vacuum not perfect, as denoted by the form of stratification, but in this tube the colour denotes that even a trace of air remains,—probably that portion in the narrow part of the siphon-gauge, which, from its position, was not displaced by the carbonic acid.

The potash was subsequently heated until the discharge was reduced to a wave-line, with very narrow striæ; in this state moisture is seen adhering to the sides of the tube; but even in this state the difference in the level of the mercury in the gauge did not ever vary more than ·05 inch. As the potash cooled, the discharge altered

through all the well-known phases of the striæ, the mercury again becoming quite level.

At first almost the slightest heat applied to the potash alters the form of the stratifications; as the heating is repeated, longer application is necessary; but it shows how sensibly the electrical discharge denotes the perfection of a vacuum, which cannot be detected by the ordinary method of mercurial siphon-gauge.

Jan. 26.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communications were read:—

“On the alteration of the Pitch of Sound by conduction through different Media.” By Sydney Ringer, Esq.

“On the frequent occurrence of Phosphate of Lime, in the crystalline form, in Human Urine, and on its pathological importance.” By Arthur Hill Hassall, M.D. Lond.

The author concludes from his observations and investigations:—

First. That deposits of *crystallized* phosphate of lime are of frequent occurrence in human urine, much more so, indeed, than those of the amorphous or granular form of that phosphate.

Second. That the crystals present well-marked and highly characteristic forms, whereby the identification of this phosphate by means of the microscope is rendered easy and certain.

Third. That there is good reason to believe that deposits of phosphate of lime are of greater pathological importance than those of the phosphate of ammonia and magnesia.

Feb. 2.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communications were read:—

“On the Saccharine Function of the Liver.” By George Harley, M.D., F.C.S., Professor of Medical Jurisprudence in University College, London.

From his experiments the author draws the following conclusions:—

1st. Sugar is a normal constituent of the blood of the general circulation.

2ndly. Portal blood of an animal on mixed diet contains sugar.

3rdly. Portal blood of a fasting animal, as well as of an animal fed solely on flesh, is devoid of sugar.

4thly. The livers of dogs contain sugar, whether the diet is animal or vegetable.

5thly. Under favourable circumstances, saccharine matter may be found in the liver of an animal after three entire days of rigid fasting.

6thly. The sugar found in the bodies of animals fed on mixed food is partly derived directly from the food, partly formed in the liver.

7thly. The livers of animals restricted to flesh diet possess the power of forming glucogen, which glucogen is at least in part transformed into sugar in the liver;—an inference which does not exclude the probability of glucogen (like starch in the vegetable organism) being transformed into other materials besides sugar.

8thly. As sugar is found in the liver at the moment of death, its presence cannot properly be ascribed to a *post mortem* change, but is to be regarded as the result of a natural condition.

“Hereditary Transmission of an Epileptiform Affection accidentally produced.” By E. Brown-Séquard, M.D.

Feb. 9.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communications were read:—

“On the Resin of the *Ficus rubiginosa*, and a new Homologue of Benzylic Alcohol.” By Warren De la Rue, Ph.D., F.R.S., and Hugo Müller, Ph.D., F.C.S.

In this communication the authors give an account of a new alcohol homologous with benzylic alcohol ($C_{14}H_8O_2$) which they have found occurring in the state of a natural acetic ether in the exudation from an Australian plant known as the *Ficus rubiginosa*.

This acetic ether, for which they propose the name of Acetate of Sycoceryle, constitutes about 14 per cent. of the crude resin; the remainder consisting principally of an amorphous resin which they name Sycoretine.

The different degree of solubility of the various constituents in alcohol, afforded the means of the separation of the one from the other; none of them present any remarkable properties except the new ether; so that the authors have devoted their attention mainly to the working out of the chemical relations of this substance.

Acetate of sycoceryle, having very characteristic properties, could be readily obtained in beautiful crystals; but some difficulty occurred in obtaining it absolutely pure, on account of the presence of a parasitical body which accompanied it constantly in solution, and always crystallized upon it. At last means were found of removing the latter substance by dissolving out the acetate of sycoceryle with ether. The per-centage composition of this parasitical body was found to be—

Carbon	76·56
Hydrogen	12·30
Oxygen	11·24

but it existed in too small a quantity to admit of its true chemical relations being made out.

Acetate of sycoceryle gave on analysis the following per-centages as the mean result of two accordant analyses:—

Carbon	79·09
Hydrogen	10·28
Oxygen	10·63

These numbers agree well with those required by the formula $C_{40}H_{32}O_4$ based upon experimental evidence.

Acetate of sycoceryle, when acted upon by sodium-alcohol, yielded acetic acid and a beautiful crystalline body resembling caffeine or asbestos; this proved to be a new member of the benzylic alcohol series having the composition $C_{36}H_{30}O_2$, which requires the following per-centage quantities:—

		Mean of two analyses.
Carbon	82.44	82.39
Hydrogen	11.45	11.38
Oxygen	6.11	6.23

The authors, by acting with chloride of benzoyl on sycocerylic alcohol, obtained the corresponding benzoate of sycoceryle; and by employing chloride of ethyle (acetyl), have prepared the acetate of sycoceryle which was identical with the original crystalline constituent of the resin.

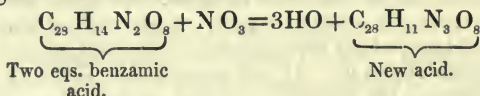
By treating sycocerylic alcohol with nitric acid, an acid was procured which appears to be sycocerylic acid.

The products of the action of chromic acid on sycocerylic alcohol, were a white crystalline neutral substance and a body crystallizing in large flat prisms. The latter appears to be the sycocerylic aldehyde.

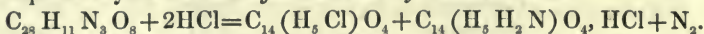
“Analytical and Synthetical Attempts to ascertain the cause of the differences of Electric Conductivity discovered in Wires of nearly pure Copper.” By Professor William Thomson, F.R.S.

“On a new Method of Substitution; and on the formation of Iodobenzoic, Iodotoluylic, and Iodoanisic Acids.” By P. Griess, Esq.

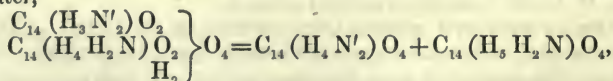
In a previous notice* I have pointed out the existence of a new class of nitrogenous acids which are generated by the action of nitrous acid on the amidic acids of the benzoic group, the change consisting in the substitution of one equivalent of nitrogen for three equivalents of hydrogen in two molecules of the amidic acid.



Under the influence of various agents these new acids undergo remarkable changes, amongst which the transformation produced by the mineral acids deserves to be particularly noticed. If the acid $\text{C}_{28}\text{H}_{11}\text{N}_3\text{O}_8$ be gently heated with strong hydrochloric acid, nitrogen gas is evolved, the yellow colour of the original acid disappears, and a red body separates, which may be separated by filtration and purified by treatment with animal charcoal. Both the physical properties and the analysis of the substance thus obtained, prove it to be pure *chlorobenzoic acid*. The hydrochloric mother-liquor on evaporation deposits crystals of the *hydrochlorate of benzamic acid*.



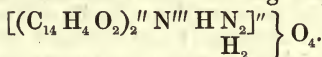
To render intelligible this transformation, the acid $\text{C}_{28}\text{H}_{11}\text{N}_3\text{O}_8$ may be viewed as a double acid corresponding to two molecules of water,



and splitting under the influence of hydrochloric acid into the two

* Phil. Mag. vol. xvii. p. 370.

groups $C_{14}(H_4 N'_2)O_4$ and $C_{14}(H_5 H_2 N)O_4$, in the first of which the two equivalents of monatomic nitrogen are replaced by hydrochloric acid, producing $C_{14}(H_5 Cl)O_4$, while the second simply combines with hydrochloric acid, producing hydrochlorate of benzoic acid. It deserves to be mentioned that the acid $C_{28}H_{11}N_3O_8$ may be derived also from two equivalents of hydrated oxide of ammonium, when its formula assumes the following shape:—



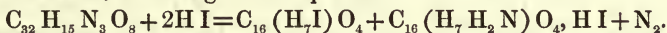
Further experiments are necessary to decide which of these two formulæ deserves the preference.

Iodobenzoic Acid, $C_{14}(H_5 I)O_4$.

This substance is produced by a process similar to that which furnishes the chlorobenzoic acid, viz. by the action of hydriodic acid on the acid $C_{28}H_{11}N_3O_8$,—beautiful white plates resembling benzoic acid, easily soluble in alcohol and in ether and difficultly soluble in water. Iodobenzoic acid is remarkable for its great stability; even fuming nitric acid fails to expel the iodine, and transforms the substance simply into nitro-iodobenzoic acid. The silver salt of iodobenzoic acid is a white amorphous precipitate containing $C_{14}(H_4 I Ag)O_4$.

Iodotoluic Acid, $C_{16}(H_7 I)O_4$.

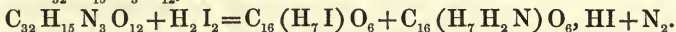
This acid is formed from the analogous nitrogenous acid in the toluic series, according to the equation



It crystallizes in white plates of a pearly lustre, which in their chemical and physical properties are very similar to iodobenzoic acid.

Iodoanisic Acid, $C_{16}(H_7 I)O_6$

is obtained by the action of hydriodic acid upon the nitrogenous acid $C_{32}H_{15}N_3O_{12}$,



Exceedingly small, nearly white needles, almost insoluble in boiling water, very soluble in alcohol and in ether.

The new method of substitution, by which the described products were obtained, although less direct than the ordinary processes, promises nevertheless to adapt itself to several cases of special interest. I am at present engaged in pursuing these experiments, with the view of producing fluo- and cyano-benzoic acids and their homologues, which have never been obtained.

The experiments which I have described were performed in Professor Hofmann's laboratory.

Feb. 16.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communications were read:—

“Description of an Instrument combining in one a Maximum and Minimum Mercurial Thermometer, invented by Mr. James Hicks.” By Balfour Stewart, Esq.

About a fortnight since, Mr. James Hicks, the intelligent foreman

of Mr. L. P. Casella, Optician, called at Kew Observatory with an instrument of the above description, for the purpose of having it compared with the ordinary maximum and minimum thermometers. This comparison proving very satisfactory, and the principle of the instrument commending itself to Dr. Robinson, Mr. Gassiot, Professor Walker, and several other scientific men who examined it, Mr. Gassiot requested me to write a short description of it, which he thought might be of interest to the Royal Society. For many particulars of this description I am indebted to Mr. Casella and Mr. Hicks, who furnished me with details regarding the construction of the instrument.

Its chief advantage consists in its furnishing us with a mercurial minimum thermometer, no serviceable instrument of this description having hitherto been made. At the same time it is also capable of being used as a mercurial maximum thermometer.

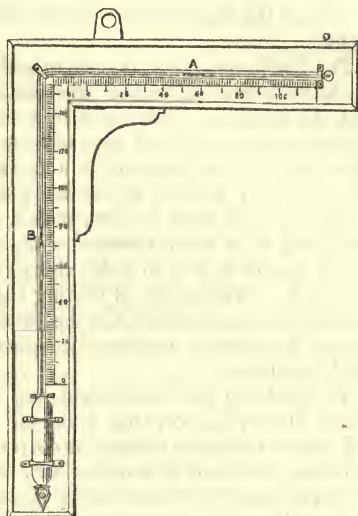
The principle of the instrument is briefly as follows:—

It has a cylindrical bulb nearly $3\frac{1}{2}$ inches long and half an inch in diameter, filled with mercury. This gives a bore nearly $\frac{1}{20}$ th of an inch wide, and a scale on which 1° Fahr. corresponds to about $\frac{1}{20}$ th of an inch. When the graduation has reached 150° Fahr. or so, both the tube and the scale are made to assume a position at right angles to that which they occupied previously, so that the first portion of the thermometer being vertical, the second will be horizontal. The numbers on the horizontal scale are not, however, in continuation of those on the vertical; for in the instrument from which this account is taken, while 150° is the highest division on the vertical scale, the first on the horizontal is -10° , the next 0° , the 3rd 10° , and so on. The reason of this method of graduation will immediately appear.

Above the mercury there is a small quantity of spirits of wine which extends some distance into the horizontal tube. The quantity of this, and the graduation, correspond in such a manner, that the extreme end of the spirit column denotes the same degree of temperature as the mercury. The remainder of the horizontal tube is filled with air. There are two moveable indices in the spirit column, one in the vertical tube, the other in the horizontal, each about half an inch long. The former, B, consists of a fine steel magnet enclosed in glass. This forms the body of the index. At either extremity there is a head of black glass, similar to that which occurs in the index of an ordinary minimum thermometer. A fine hair is tied round the neck of this index, between the body and the upper head; and it is made to hang down by the side, so that by its elastic pressure against the tube, the index may be kept in its place, notwithstanding its verticality. The index in the horizontal tube A, is in all respects similar to that of an ordinary minimum thermometer.

Let us now suppose the instrument fixed in its position, the first part of the stem being vertical. In order to adjust it, we must first bring the vertical index into contact with the upper extremity of the mercurial column. To do this, let us take two small but strong horseshoe magnets, and lay the one above the other, so that the poles of the one shall overlap to a small extent the corresponding poles of

the other. Bring the magnets up to the index in such a manner, that, while the poles of the one bear against the side of the glass tube, the overlapping poles shall lie over the tube so as to be in front of the index : the index will now follow the motion of the magnets, and it may thus be brought down to the surface of the mercury. In order to bring the horizontal index to the extremity of the spirit column, all that is necessary is to incline the horizontal tube a little downwards by pressing on the end.



The indices being now set and the instrument in adjustment, let us suppose the temperature to rise ; the mercurial column will push the vertical index up, but this index will remain in its place when the mercury again falls, and will therefore denote the maximum temperature reached. On the other hand, let us suppose the temperature to fall. The mercury in falling is followed by the spirit column propelled by the air behind it. The spirit column, again, will, on its edge coming in contact with the end of the horizontal index draw the index with it into a position, where it will remain when the mercury again rises. This index will therefore register the extreme minimum point which the spirit column has reached ; but, by the principle of graduation, this will correspond with the minimum point reached by the mercurial column.

Let us now suppose that a small portion of the spirit column has become separated, and lodged itself in the extremity of the tube. The principle of graduation will immediately enable us to discover this, by a want of correspondence being produced in the readings of the mercurial and of the spirit column. If, for instance, before the separation, the mercury read 50° , and the horizontal extremity of the spirit column also 50° , it is clear that, after the abstraction of spirits has taken place, the horizontal column will read lower.

We have thus a check upon this possible source of error, which we have not in the ordinary minimum thermometer. Indeed, it is to all intents a mercurial minimum thermometer that we are now describing, the spirits serving merely as a vehicle for the indices. It will be remarked, that were both columns capable of acting in a horizontal position, there would be no necessity for the bend, and the instrument would be more portable ; but in this position it is found that there is danger of the spirits becoming mixed with the mercury, and thus interfering with the action of the instrument. Should this ever be brought about by travelling, or any other cause, a smart jerk

or two of the instrument will join the separated columns and put all right.

The instrument is thus constructed :—The vertical tube, including the bulb, is first made and filled with mercury to the proper height, and the magnetic index is introduced ; then the horizontal tube is joined, and the spirits of wine and the horizontal index are introduced. The bulb is then placed in a freezing mixture, in order that the mercury may retreat as far as possible, followed by the spirits of wine. The tube is then sealed, care being taken that the bore shall end in a small rounded chamber ; for if pointed, some of the spirits would be apt to lodge there, whence it would be difficult to remove it. The object of cooling the bulb before sealing off, is that we may have as much air in the tube as possible ; for its pressure, as already mentioned, enables the spirits to follow the mercury when the latter falls.

To graduate the instrument, set it with the mercurial stem horizontal in melting ice, then point off the extremity of the mercurial, and also of the spirit column as corresponding to 32° Fahr. Perform a similar operation in water at 42°, 52°, 62°, &c., and also in freezing mixtures down to zero, or lower if necessary.

In conclusion, if used as a wet-bulb thermometer, this instrument will give us the maximum and minimum temperatures of evaporation obtained under precisely the same circumstances.

“On the Expansion of Metals and Alloys.” By F. Crace-Calvert, Esq., F.R.S., and G. Cliff Lowe, Esq.

One of us having been engaged for some time in investigating several of the properties of pure metals, it was thought desirable to take advantage of having pure metals at our disposal, together with a series of definite alloys of those metals, to determine their rate of expansion. And we were encouraged in pursuing this course of investigation, by finding that several of the authors who had previously published tables of the expansion of metals differed widely in their results. These discrepancies, having reference to some of the metals most extensively used, might, we thought, be due either to the method employed, or to the fact that metals of different degrees of purity had been experimented upon. Therefore, being sure of the purity of the metals that we intended to employ, we had recourse to a method the accuracy of which we trust will appear satisfactory.

Owing to the difficulty of obtaining the metals in a pure state in large quantities, we found it necessary to employ square bars, having a length of 60 millimetres by 10 millimetres of diameter. We therefore devised a process to determine with accuracy the expansion of such short bars. This, we believe, we have effected, as our apparatus will easily indicate an expansion amounting to the 50,000th of an inch, or about the 2000th part of a millimetre.

Omitting the description of our apparatus and of the details of our operations, which would be long for this abstract of our results, we give here a Table of the general results obtained with the following metals :—

	Divisions of the scale read off in 25,000ths of an inch in a rising temperature from 10° to 90°.				Divisions of same scale read off in cooling from 90° to 10°.				Mean for 100° C. calculated from these means and corrected for expansion of vessel, &c., by deducting 20.
	1st.	2nd.	3rd.	Mean.	1st.	2nd.	3rd.	Mean.	
Cadmium	174	171	172	172·3	176	173	172	173·7	196·2
Lead	155	156	157	156	161	160	159	160·0	177·5
Tin	142	142	145	143	147	148	147	147·3	161·5
Aluminium.....	120	120	120	120	122·5	121	122	121·8	131·1
Forged zinc	119	121	120	120	120	119	120	119·7	129·8
Silver	110	109	109·5	109·5	111·5	110	110	110·5	117·5
Copper (pure) cast	106	105·5	106	105·8	103	103·5	107	104·5	111·4
Copper (pure) } hammered... }	99	99	99	99	101	99	100	100	104·4
Gold	81	80·5	80·7	81·5	81	81·3	81·3
Bismuth	78	77	77·5	77·5	81·5	80	80	80·5	78·8
Wrought iron.....	69	72	73	71·3	73·5	72	72·5	72·7	70·0
Cast iron.....	67	68·5	68·5	68·0	68	70·5	70·5	69·7	66·1
Steel (soft).....	66·5	62·0	63	63·8	66·5	65	66·5	66	61·1
Antimony	63	62	62·5	63	62	62·5	58·1
Platinum	57·5	57·5	58	58·0	52·2

From the above observations we deduce the following Table of coefficients of linear expansion from 0° to 100°:—

Cadmium (pure)	0·00332
Lead (pure)	0·00301
Tin (pure)	0·00273
Aluminium (commercial)	0·00222
Zinc, forged (pure)	0·00220
Silver (pure)	0·00199
Gold (pure)	0·00138
Bismuth (pure)	0·00133
Wrought iron	0·00119
Cast iron	0·00112
Steel (soft)	0·00103
Antimony (pure)	0·00098
Platinum (commercial)	0·00068

On comparing these coefficients with those found by previous experimenters, we find that they agree very closely in those cases where commercial metals have been employed. But when we come to those metals which we employed in a pure state, such as lead, tin, zinc, silver, copper, bismuth, antimony, cadmium, and gold, we find a marked difference, which we attribute to our experiments having been made with pure metals; and we are confirmed in this view by several series of experiments made with impure or commercial metals.

We give in our paper several series of experiments which prove

that, as for conductibility of heat by bodies, their molecular condition exercises the greatest influence on their expansion. For example, we have found that the same bar of steel gives, according to its degree of tempering, the following ratios of expansion :—

	Raising temperature from 10° to 90°.				Cooling temperature from 90° to 10°.				Mean calculated and corrected for a bar 60 mm. for 100°.
	1st.	2nd.	3rd.	Mean.	1st.	2nd.	3rd.	Mean.	
Steel bar as purchased	111	112	111·5	111·5	113	113	115	113·7	64·6
Steel bar at maximum of softness	107	108	107·5	107·5	107	112	111	110	62·5
Same bar at maximum of hardness	141	145	140	142	138	139	139	138·7	84·0

The influence of the molecular state of bodies is also clearly illustrated in the class of compounds or carbonates of lime :—

	Rates of expansion from 0° to 100°.				
Chalk	19·6
Lithographic stone	45·0
Stalactite	67·0
Marble	71·0

Influence of Crystallization.

Crystallization influences the expansion of bodies as it does their power to conduct heat; thus, the same zinc cast horizontally or vertically has not only a different crystallized structure, but also expands in a different ratio.

	Raising temperature 10° to 90°.					Cooling temperature 90° to 10°.				Mean for 100° less correc- tion.
	1st.	2nd.	3rd.	4th.	Mean.	1st.	2nd.	3rd.	Mean.	
Zinc cast vertically	224	226	227	226	226	232	233	234	233	266·9
Zinc cast hori- zontally	187	186·5	187	186·8	190·5	193	192·5	192	216·7

We have also examined the ratio of expansion in several series of alloys made in multiple and definite proportions, but shall give here only one series as illustrating our results.

Copper and Tin.

	10° to 90°.				Mean for 100° less correction.					Mean for 100° less correction.
	1st.	2nd.	3rd.	Mean.		1st.	2nd.	3rd.	Mean.	
5Sn 90·27 } 1Cu 9·73 }	127	124	124	125	136·2	129	124	126·5	138·1
4Sn 88·14 } Cu 11·86 }	122	122	122·0	132·5	127	126	126·5	138·1
3Sn 84·79 } Cu 15·21 }	119	119·5	119·2	129·5	123	122	123·5	122·8	133·5
2Sn 78·79 } Cu 21·21 }	118	118	118	127·5	117	117	117	126·2
Sn 65·02 } Cu 34·98 }	109·5	111·5	110·5	118·1	110·5	110·5	110·5	118·1
Sn 48·17 } 2Cu 51·83 }	111·0	111	111	118·7	113·5	112	112·7	120·8
Sn 34·21 } 3Cu 61·79 }	112	111	111·5	119·3	113	112	112·5	120·6
Sn 31·73 } 4Cu 68·27 }	102	105	103·5	109·3	106	105	105·5	111·8
Sn 27·10 } 5Cu 72·90 }	104	104	104	110	106	106	106	112·5
Sn 15·68 } 10Cu 84·32 }	101	101	101	106·2	101	101	101	106·2
Sn 11·03 } 15Cu 88·97 }	95	94·5	94·7	98·3	94·5	95	94·7	98·3
Sn 8·51 } 20Cu 91·49 }	97	99·5	98·2	102·7	99	99	99	103·7
Sn 6·83 } 25Cu 93·17 }	99	99	99	103·7	100	100	100	105

Feb. 23.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communication was read:—

“Measurement of the Electrostatic Force produced by a Daniell’s Battery.” By Professor William Thomson, F.R.S.

In a paper “On Transient Electric Currents,” published in the Philosophical Magazine for June 1853, I described a method for measuring differences of electric potential in absolute electrostatic units, which seemed to me the best adapted for obtaining accurate results. The “absolute electrometer” which I exhibited to the British Association on the occasion of its meeting at Glasgow in 1855, was constructed for the purpose of putting this method into practice, and, as I then explained, was adapted to reduce the indications of an electroscopic * or of a torsion electrometer to absolute measure.

The want of sufficiently constant and accurate instruments of the latter class has long delayed my carrying out of the plans then set

* I have used the expression “electroscopic electrometer,” to designate an electrometer of which the indications are merely read off in each instance by a single observation, without the necessity of applying any experimental process of weighing, or of balancing by torsion, or of otherwise modifying the conditions exhibited.

forth. Efforts which I have made to produce electrometers to fulfil certain conditions of sensibility, convenience, and constancy, for various objects, especially the electrostatic measurement of galvanic forces, and of the differences of potential required to produce sparks in air, under definite conditions, and the observation of natural atmospheric electricity, have enabled me now to make a beginning of absolute determinations, which I hope to be able to carry out soon in a much more accurate manner. In the meantime I shall give a slight description of the chief instruments and processes followed, and state the approximate results already obtained, as these may be made the foundation of various important estimates in several departments of electrical science.

The absolute electrometer alluded to above, consists of a plain metallic disc, insulated in a horizontal position, with a somewhat smaller plane metallic disc hung centrally over it, from one end of the beam of a balance. A metal case protects the suspended disc from currents of air, and from irregular electric influences, allowing a light vertical rod, rigidly connected with the disc at its lower end, and suspended from the balance above, to move up and down freely, through an aperture just wide enough not to touch it. In the side of the case there is another aperture, through which projects an electrode rigidly connected with the lower insulated disc. The upper disc is kept in metallic communication with the case.

In using this instrument to reduce the indications of an electroscopic or torsion electrometer to absolute electrostatic measure, the insulated part of the electrometer is kept in metallic communication with the insulated disc, while the cases enclosing the two instruments are also kept in metallic communication with one another. A charge, either positive or negative, is communicated to the insulated part of the double apparatus. The indication of the tested electrometer is read off, and at the same time the force required to keep the moveable disc at a stated distance from the fixed disc below it, is weighed by the balance. This part of the operation is, as I anticipated, somewhat troublesome, in consequence of the instability of the equilibrium, but with a little care it may be managed with considerable accuracy. The plan which I have hitherto followed, has been to limit the play of the arm of the balance to a very small arc, by means of firm stops suitably placed, thus allowing a range of motion to the upper disc through but a small part of its whole distance from the lower. A certain weight is put into the opposite scale of the balance, and the indications of the second electrometer are observed when the electric force is just sufficient to draw down the upper disc from resting in its upper position, and again when insufficient, to keep it down with the beam pressed on its lower stop. This operation is repeated at different distances, and thus no considerable error depending on a want of parallelism between the discs could remain undetected. It may be remarked that the upper disc is carefully balanced by means of small weights attached to it, so as to make it hang as nearly as possible parallel to the lower disc. The stem carrying it is graduated to hundredths of an inch; and by watching it through a

telescope at a short distance, it is easy to observe $\frac{1}{1000}$ th of an inch of its vertical motion.

I have recently applied this method to reduce to absolute electrostatic measure the indications of an electrometer forming part of a portable apparatus for the observation of atmospheric electricity. In this instrument a very light bar of aluminium attached at right angles to the middle of a fine platinum wire, which is firmly stretched between the inside coatings of two Leyden phials, one occupying an inverted position above the other, experiences and indicates the electrical force which is the subject of measurement, and which consists of repulsions in contrary directions on its two ends, produced by two short bars of metal fixed on the two sides of the top of a metal tube, supported by the inside coating of the lower phial.

The amount of the electrical force (or rather as it should be called in correct mechanical language, *couple*) is measured by the angle through which the upper Leyden phial must be turned round an axis coincident with the line of the wire, so as to bring the index to a marked position. An independently insulated metal case, bearing an electrode projecting outwards, to which the body to be tested is applied, surrounds the index and repelling bars, but leaves free apertures above and below, for the wire to pass through it without touching it; and by other apertures in its sides and top, it allows the motions of the index to be observed, and the Leyden phials to be charged or discharged at pleasure, by means of an electrode applied to one of the fixed bars described above. When by means of such an electrode the inside coatings of the Leyden phials are kept connected with the earth, this electrometer becomes a plain repulsion electrometer, on the same principle as Peltier's, with the exception that the index, supported by a platinum wire instead of on a pivot, is directed by elasticity of torsion instead of by magnetism; and the electrical effect to be measured is produced by applying the electrified body to a conductor connected with a fixed metal case round the index and repelling bars, instead of with these conductors themselves.

This electrometer, being of suitable sensibility for direct comparison with the absolute electrometer according to the process described above, is not sufficiently sensitive to measure directly the electrostatic effect of any galvanic battery of fewer than two hundred cells with much accuracy. Not having at the time arrangements for working with a multiple battery of reliable character, I used a second torsion electrometer of a higher degree of sensibility as a medium for comparison, and determined the value of its indications by direct reference to a Daniell's battery of from six to twelve elements in good working order. This electrometer, in which a light aluminium index, suspended by means of a fine glass fibre, kept constantly electrified by means of a light platinum wire hanging down from it and dipping into some sulphuric acid in the bottom of a charged Leyden jar, exhibits the effects of electric force due to a difference of potentials between two halves of a metallic ring separately insulated in its

neighbourhood, will be sufficiently described in another communication to the Royal Society. Slight descriptions of trial instruments of this kind have already been published in the Transactions of the Pontifical Academy of Rome*, and in the second edition of Nichol's Cyclopædia (article Electricity, Atmospheric), 1860.

I hope soon to have another electrometer on the same general principle, but modified from those hitherto made, so as to be more convenient for accurate measurement in terms of constant units. In the meantime I find, that, by exercising sufficient care, I can obtain good measurements by means of the divided ring electrometer of the form described in Nichol's Cyclopædia.

In the ordinary use of the portable electrometer, a considerable charge is communicated to the connected inside coatings of the Leyden phials, and the aluminium index is brought to an accurately marked position by torsion, while the insulated metal case surrounding it is kept connected with the earth. The square root of the reading of the torsion-head thus obtained measures the potential, to which the inside coatings of the phials have been electrified. If, now, the metal case referred to is disconnected from the earth and put in connexion with a conductor whose potential is to be tested, the square root of the altered reading of the torsion-head required to bring the index to its marked position in the new circumstances measures similarly the difference between this last potential and that of the inside coatings of the phials. Hence the excess of the latter square root above the former expresses in degree and in quality (positive or negative) the required potential. This plan has not only the merit of indicating the quality of the electricity to be tested, which is of great importance in atmospheric observation, but it also affords a much higher degree of sensibility than the instrument has when used as a plain repulsion electrometer; and, on account of this last-mentioned advantage, it was adopted in the comparisons with the divided ring electrometer. On the other hand, the portable electrometer was used in its least sensitive state, that is to say, with its Leyden phials connected with the earth, when the comparisons with the absolute electrometer were made.

The general result of the weighings hitherto made, is that when the discs of the absolute electrometer were at a distance of twenty hundredths of an inch, the number of degrees of torsion in the portable electrometer was 3.229 times the number of grains' weight required to balance the attractive force; and the number of degrees of torsion was 7.69 times the number of grains' weight found in other series of experiments in which the distance between the discs was thirty hundredths. According to the law of inverse squares of the distances to which the attraction between two parallel discs is subject when a constant difference of potentials is maintained between them†, the force at a distance of $\frac{1}{10}$ of an inch would have been $\frac{1}{807}$, according to the first of the preceding results, or, according to

* Accademia Pontificia dei Nuovi Lyncei, February 1857.

† See § 11 of elements of mathematical theory of electricity appended to the communication following this in the 'Proceedings.'

the second, $\frac{1}{854}$ of the number of degrees of torsion. The mean of these is $\frac{1}{83}$, or 1.2; and we may consider this number as representing approximately the value in grains' weight at $\frac{1}{10}$ of an inch distance between the discs of the absolute electrometer, corresponding to one degree of torsion of the portable electrometer. By comparing the indications of the portable electrometer with those of the divided ring electrometer, and by evaluating those of the latter in terms of the electromotive force of a Daniell's battery charged in the usual manner, I find that 284 times the square root of the number of degrees of torsion in the portable electrometer is approximately the number of cells of a Daniell's battery which would produce an electromotive force (or, which is the same thing, a difference of potentials) equal to that indicated. Hence the attraction between the discs of the portable electrometer, if at $\frac{1}{10}$ of an inch distance, and maintained at a difference of potentials amounting to that produced by 284 cells, is 1.2 grain. The effect of 1000 cells would therefore be to give a force of 14.9 grains, since the force of attraction is proportional to the square of the difference of potentials between the discs. The diameter of the opposed circular areas between which the attraction observed took place, was 5.86 inches. Its area was therefore .187 of a square foot, and therefore the amount of attraction per square foot, according to the preceding estimate for $\frac{1}{10}$ of an inch distance and 1000 cells' difference of potential, is 79.5 grains. To reduce the statement to consistent units founded on a foot as the unit of length, we may suppose, instead of $\frac{1}{10}$ of an inch, the distance between the discs to be $\frac{1}{100}$ of a foot. We conclude that, with an electromotive force or difference of potentials produced by 1000 cells of Daniell's battery, we find for the force of attraction 55.3 grains per square foot between discs separated to a distance of $\frac{1}{100}$ of a foot.

This result differs very much from an estimate I have made according to my theoretical estimate of 2,500,000 British electromagnetic units for the electromotive force of a single element of Daniell's and Weber's comparison of electrostatic with electromagnetic units. On the other hand, it agrees to a remarkable degree of accuracy with direct observations made for me, during my absence in Germany, by Mr. Macfarlane, in the months of June and July 1856, on the force of attraction produced by the direct application of a miniature Daniell's battery, of different numbers from 93 to 451 of elements, applied to the same absolute electrometer with its discs at .079 of an inch asunder. These observations gave forces varying, on the whole, very closely according to the square of the number of cells used; and the mean result reduced according to this law to 1000 cells was 23.4 grains. Reducing this to the distance $\frac{1}{100}$ of a foot, and dividing by .187, the area in decimal of a square foot, we find 54.3 grains per square foot at a distance of $\frac{1}{100}$ of a foot.

Although the experiments leading to this result were executed with great care by Mr. Macfarlane, I delayed publishing it because of the great discrepancy it presented from the estimate I deduced from Weber's measurement, which was published while my preparations

were in progress. I cannot doubt its general correctness now, when it is so decidedly confirmed by the electrometric experiments I have just described, which have been executed chiefly by Mr. John Smith and Mr. John Ferguson, working in my laboratory with much ability since the month of November. I am still unable to explain the discrepancy, but it may possibly be owing to some miscalculation I have made in my deductions from Weber's result.

Glasgow College, Jan. 18, 1860.

POSTSCRIPT, April 12, 1860.

I have since found that I had inadvertently misinterpreted Weber's statement in the ratio of 2 to 1. I had always, as it appears most natural to me to do, regarded the transference of negative electricity in one direction and of positive electricity in the other direction, as identical agencies, to which in our ignorance as to the real nature of electricity we may apply indiscriminately the one expression or the other, or a combination of the two. Hence I have always regarded a current of unit strength as a current in which the positive or vitreous electricity flows in one direction at the rate of a unit of electricity per unit of time; or the negative or resinous electricity in the other direction at the same rate; or (according to the infinitely improbable hypothesis of two electric fluids) the vitreous electricity flows in one direction at any rate less than a unit per second, and the resinous in the opposite direction at a rate equal to the remainder of the unit per second. I have only recently remarked that Weber's expressions are not only adapted to the hypothesis of two electric fluids, but that they also reckon as a current of unit strength, what I should have called a current of strength 2, namely a flow of vitreous electricity in one direction at the rate of a unit of vitreous electricity per unit of time, and of the resinous electricity in the other direction simultaneously, at the rate of a unit of resinous electricity per unit of time.

Weber's result as to the relation between electrostatic and electromagnetic units, when correctly interpreted, I now find would be in perfect accordance with my own results given above, if the electromotive force of a single element of the Daniell's battery used were 2,140,000 British electro-magnetic units instead of 2,500,000, as according to my thermo-dynamic estimate. This is as good an agreement as could be expected when the difficulties of the investigations, and the uncertainty which still exists as to the true measure of the electromotive force of the Daniell's element are considered. It must indeed be remarked that the electromotive force of Daniell's battery varies by two or three or more per cent. with variations of the solutions used; that it varies also very sensibly with temperature; and that it seems also to be dependent, to some extent, on circumstances not hitherto elucidated. A thorough examination of the electromotive force of Daniell's and other forms of galvanic battery, is an object of high importance, which it is to be hoped will soon be attained. Until this has been done, at least for Daniell's battery, the results of the preceding paper may be regarded as having about as much accuracy as is desirable.

I may state therefore, in conclusion, that the average electromotive force per cell of the Daniell's batteries which I have used, produces a difference of potentials amounting to '0021 in British electrostatic measure. This statement is perfectly equivalent to the following in more familiar terms:—

One thousand cells of Daniell's battery, with its two poles connected by wires with two parallel plates of metal $\frac{1}{100}$ th of a foot apart and each a square foot in area, produces an electrical attraction equal to the weight of 55 grains.

GEOLOGICAL SOCIETY.

[Continued from p. 166.]

May 16, 1860.—L. Horner, Esq., President, in the Chair.

The following communication was read:—

“On the co-existence of Man with certain Extinct Quadrupeds, proved by Fossil Bones, from various Pleistocene Deposits, bearing Incisions made by sharp instruments.” By M. E. Lartet, For.M.G.S. In a Letter to the President.

The author, having for some time past made observations upon fossil bones exhibiting evident impressions of human agency, was requested by the President, who had examined the specimens indicated, to communicate the results of his researches to this Society.

The specimens referred to are:—1st, fragments of bones of *Aurochs* exhibiting very deep incisions, made apparently by an instrument having a waved edge; 2ndly, a portion of a skull of *Megaceros hibernicus*, bearing significant marks of the mutilation and flaying of a recently slain animal. These were obtained from the lowest layer in the cutting of the Canal de l'Ourcq, near Paris, and have been figured by Cuvier in his 'Ossem. Foss.' Molars of *Elephas primigenius* found in the same deposit are figured by Cuvier, who states that they had not been rolled, but had been deposited in an original and not a *remanié* deposit. 3rdly. Among bones, with incisions, from the sands of Abbeville, are a large antler of an extinct stag (*Cervus Sommenensis*) and several horns of the common Red-Deer. 4thly. Bones of *Rhinoceros tichorhinus* from Menchecourt, near Abbeville, where flints worked by human hands have been found. 4thly. Portions of horns of *Megaceros* from the British Isles. In reference to the remains of the Gigantic Deer, M. Lartet alludes to the Rev. J. G. Cumming's statement that stone implements have been found in the Isle of Man imbedded with remains of the *Megaceros*, and that hatchet-marks have been seen on an oak-tree in a submerged forest of possibly still older date. 5thly. Fragments of bone collected by M. Delesse from a deposit near Paris, and exhibiting evidence of having been sawn, not with a smooth metallic saw, but with such an instrument as the flint knives or splinters, with a sharp chisel-edge, found at Abbeville would supply.

If, says the author, the presence of worked flints in the gravel and sands of the valley of the Somme have established with certainty the existence of man at the time when those very ancient deposits

were formed, the traces of an intentional operation on the bones of *Rhinoceros*, *Aurochs*, *Megaceros*, *Cervus Somenensis*, &c. supply equally the inductive demonstration of the contemporaneity of those species with the human race. M. Lartet points out that the Aurochs, though still existing, was contemporaneous with the *Elephas primigenius*, and that its remains occur in preglacial deposits, and, indeed, that a great proportion of our living Mammifers have been contemporaneous with *E. primigenius* and *R. tichorhinus*, the first appearance of which in Western Europe must have been preceded by that of several of our still existing quadrupeds.

The author accepts M. d'Archiac's determination of the period of the separation of England from the Continent as having been anterior to the formation of the ancient alluvium or "loess," but subsequent to the great rolled gravel-deposits in which the flint hatchets of a primitive people are found. If M. E. de Beaumont's hypothesis of these gravels being due to the last dislocation of the Alps be accepted, the worked flints carried along with the erratic pebbles afford a proof of the existence of man at an epoch when Central Europe had not yet fully received its present geographical features.

The author also remarks that though there is good evidence of changes of level having occurred since man began to occupy Europe and the British Isles, yet they have not amounted to catastrophes so general as to affect the regular succession of organized beings.

Lastly, M. Lartet announced that a flint hatchet and some flint knives had lately been discovered, in company with remains of Elephant, Aurochs, Horse, and a feline animal, in the sands of the Parisian suburb of Grenelle, by M. Gosse, of Geneva.

May 30, 1860.—L. Horner, Esq., President, in the Chair.

The following communication was read:—

"On certain Rocks of Miocene and Eocene age in Tuscany, including Serpentine, accompanied by Copper-ore, Lignite, and Alabaster." By W. P. Jervis, Esq., F.G.S.

Three distinct eruptions of serpentinous igneous rocks have been recognized by the Italian geologists; two are considered to have occurred in Tertiary times, and one previously in the mesozoic period; dykes of diorite (also of Tertiary age) are more rare in the same geographical area. From the abundant occurrence of these eruptive rocks, and the extensive development of Miocene strata, unknown in England, arise many specialities of Tuscan geology and mineralogy. 1st. The diallagic serpentine has pierced the Upper Cretaceous beds, but does not enclose any fragments of Tertiary rocks. It is non-metalliferous, and is employed in architecture. 2ndly. The euphotide or "granitone" is unfit for building-purposes. The contact of this with the diallagic serpentine has metamorphosed the latter into the curiously marked "Ranocchiaja." At Matarana (Liguria) the crystals of diallage in the euphotide gradually decrease in size towards the junction with the serpentine. 3rdly. Diorite, penetrating the euphotide, and, like it, belonging to the Eocene age. This and the serpentine acting on the "Macigno" has produced the "Gabbro rosso." 4thly. "Gabbro verde," or serpentine,

without diallage, of Miocene age. This is much softer than the diallagic serpentine. It forms dykes; but more generally it is the axial nucleus of hills and mountains, the strata of which are much disturbed. In most cases the serpentine rocks, piercing the sedimentary strata, have upheaved them from all sides: for this remarkable species of axis the author proposes the term *periclinal*, indicating that the strata fall off in every direction. It is frequently rich in copper-ores at its junction with the metamorphosed schists or Gabbro rosso; here also several zeolites, all containing magnesia (from 1.11 to 13.50 *per cent.*), abound; and limpid calcite in extremely obtuse rhombohedral crystals occurs. The limestones are often altered by the serpentine into dolomite (Miemmite), and are otherwise variously affected. Near Matarana a mouse-coloured limestone is changed (by the alteration of the carbonate of iron to a peroxide) into a brick-red marble, often brecciated and veined with serpentine and calc-spar ("Ofiocalce"). The copper-ores (chalcopyrites, bornite, oxide of copper, grey copper, native copper) at Monte Catini occur in nodules or boulders enveloped in the serpentine, as though they had been brought up simultaneously. Chalcedony abounds in the serpentine north of Monte Verdi, where it occurs in large veins, and is occasionally brecciated. Black flint, jasper, agate, waxy opal, &c., are not uncommon.

Pure alabaster appears to be peculiar to Western Tuscany. It occurs in ovoidal masses, often 3 feet in diameter, in selenitic marls of Miocene age in the Val di Marmolajo. Coloured alabaster is also found in some of the Pliocene beds of Tuscany. Gypsum is widely distributed where serpentine has pierced limestones, as at Matarana and Jano.

At Jano the palæozoic coal is represented by isolated plants converted into anthracite; it is the only locality on the Italian continent where Carboniferous fossils have been found; but Miocene lignites are abundant in Italy. At Sarzanello in Piedmont, 6½ feet of miocene coal occurs. This is used in the Sardinian steam-navy. At Castiani, in the Maremme, good lignite, 3 feet 4 inches thick, is worked; and at Monte Bamboli, also in Tuscany, one bed, 4 feet 2 inches, and another, 2 feet thick, have long been in use.

June 13, 1860.—L. Horner, Esq., President, in the Chair.

The following communication was read.

"On the Ossiferous Caves of the Peninsula of Gower, in Glamorganshire, South Wales." By H. Falconer, M.D., F.R.S., F.G.S. With an Appendix, on a Raised Beach in Mewslade Bay, and the occurrence of the Boulder-clay on Cefn-y-bryn; by J. Prestwich, Esq., F.R.S., Treas.G.S.

The object of this communication was to give a summary of researches made during the last three years by the author and Lieut.-Col. E. R. Wood, F.G.S., the latter of whom has carefully explored at his own charge, since 1848, some of the caves previously known, as well as several discovered by himself. The known bone-caves of Gower (of which Paviland, Spritsail Tor, and Bacon Hole have already supplied Dr. Buckland and others to some extent with ma-

terials for the history of the Cave-period) are in the Carboniferous Limestone; and, with the exception of that of Spritsail Tor, which is on the west coast of the peninsula, they all occur between the Mumbles and the Worm's Head. The most important are "Bacon Hole," "Minchin Hole," "Bosco's Den," "Bowen's Parlour," "Crow Hole," "Raven's Cliff Cavern," and lastly the well-known "Paviland Caves." Bone-caves at the Mumbles, in Caswell Bay, and in Oxwich Bay formerly existed; but the sea has destroyed them. One cavern named "Ram Tor" between Caswell Bay and the Mumbles, presumed to be ossiferous, remains unexplored.

Before proceeding to describe the bone-caves and their contents, the author briefly noticed a raised beach and talus of breccia, which Mr. Prestwich had lately traced for a mile along Mewslade Bay, westward of Paviland; and he pointed out their important relationship to the marine sands and overlying limestone-breccia found in several of the Gower Caves. Dr. Falconer also referred to Mr. Prestwich's recent discovery of some patches of Boulder-clay on the highland of Gower, and in Rhos Sili Bay.

"Bacon Hole" was first treated of. It has been worked out by Colonel Wood, and described by Mr. Starling Benson. On the limestone-floor of the cave are:—(1) a few inches of marine sand, abounding with *Litorina rudis*, *L. litoralis*, and *Clausilia nigricans*, with bones of an *Arvicola* and Birds; (2) a thin layer of stalagmite; (3) two feet or less of blackish sand, containing a mass of bones of *Elephas antiquus*, with remains of *Meles taxus* and *Putorius (vulgaris?)*; (4) one to two feet of ochreous cave-earth, limestone-breccia, and sandy layers, with remains of *Elephas antiquus*, *Rhinoceros hemitechus*, *Hyæna*, *Canis lupus*, *Ursus spelæus*, *Bos*, and *Cervus*; (5) irregular stalagmite, partly enveloping a huge tusk of an Elephant imbedded below it; (6) limestone-breccia and stalagmite, from 1 to 2 feet thick, with bones of *Ursus* and *Bos*; (7) irregular bed of stalagmite, 1 foot or more, with *Ursus*; (8) dark-coloured superficial earth, kept soppy by abundant drip, with bones of *Bos*, *Cervus*, *Canis vulpes*, horns of Reindeer and Roebuck, together with shells of *Patella*, *Mytilus*, *Purpura*, *Litorina* (probably brought into the cavern as food by birds), and also pieces of ancient British pottery. The marine sand at the bottom of "Bacon Hole" was analogous to that on the rocky floor of the San Ciro Cave, near Palermo; but contained fewer species of Mollusca. The uppermost layer of stalagmite is about 30 feet above high water. The Elephant-remains belonged to at least three individuals, one of which was adult, and one young with milk-dentition.

"Minchin Hole" is the grandest and most spacious of all the Gower Caves, being 170 feet long, by 70 feet where widest, and 35 feet high at the entrance; here the section gave—(1) Loose limestone-breccia, 3 feet; (2) Yellow cave-earth, 9 inches; (3) Sand, 1 foot; (4) Blackish sandy loam containing abundant remains of *Rhinoceros*, *Elephas*, and *Bos*, 2½ feet; (5) Greyish-yellow marine sand, varying in thickness from 1 to 4 feet, and resting on the rocky floor. Some of the lower jaws of *Rhinoceros* from this deposit exhibit *Litorina* and comminuted shells imbedded in the encrusting

matrix : and the black sand yielded *Helix hispida* similarly attached. In the interior, the cave-earth was thicker, and the black sandy loam more unctuous. The mammalian remains were closely analogous with those from Bacon Hole ; but the Elephant remains (*E. antiquus*) were fewer, and those of *Rhinoceros hemitachus* were more numerous and better preserved, including two skulls. No remains of *Elephas primigenius* or of *Rhinoceros tichorhinus* were met with in Bacon Hole or Minchin Hole.

" Bosco's Den " is a cavernous fissure, of great interest, between " Bacon Hole " and " Minchin Hole." It is about 70 feet high, and has been worked out by Col. Wood, who, having succeeded in reaching a hole called (by the quarrymen) " Bacon's Eye," found it to be an angular opening ($2\frac{1}{2}$ feet in diameter) at the top of one of the great vertical fissures in the limestone, and leading into a fine cavern. Beneath it the fissure was filled up with a mass of angular fragments of limestone (with bones, teeth, and land shells) impacted in ochreous loam, about 20 feet in height, resting on a solid platform of breccia, beneath which the fissure had to a great extent been washed out by the sea. On enlarging the aperture, by undermining the projecting mass of loam and breccia, a cavity was found extending 76 feet backwards, with a width of from 7 to 16 feet, and a general height of about 15 feet. A line of fissure runs along the angle of the roof, and towards the outer part of the cavern the crack widens into an irregular flue, which had evidently communicated with the surface : here the cavern rises to a height of 40 feet. When first opened, the eastern wall only of the cavern was found to be coated with stalagmite. The floor was tolerably smooth, and shelved down gradually from the mouth to the extremity, the deposits being thicker outwards. The floor having been excavated down to the hard breccia, there were observed :—(1) at the top, a bed of sandy peat or turf, formed chiefly of bits of sticks and comminuted vegetable matter, about 1 foot thick, except under the flue, where it formed a low conical heap. In or on this peaty covering were bones of Ox and Wolf, and bones and broken shed antlers of Deer, of species or varieties allied to the Reindeer (*Cervus Guettardi* and *Cerv. priscus*). (2) Stalagmite, regular, but usually less than a foot thick. At one spot it rose into a boss 2 ft. 3 in. high, which was found in a shattered condition, the fragments being loose, but still in place. This must indicate—1st, the operation of some shock since the formation of the stalagmite, and even since the peat began to be formed ; and 2ndly, the absence of drip in the cave since the shock took place. (3) Sandy loam, 1 ft. 4 in., with fragments of rock and without bones ; (4) sand, 2 ft. 6 in. ; (5) a bed of loose stony breccia, 4 feet, without bones ; (6) ochreous loam, or the usual cave-earth, 6 to 7 feet thick, resting on the solid cemented breccia which forms a floor or diaphragm between the upper and lower chambers of the fissure. *Ursus spelæus*, *Canis lupus*, *C. vulpes*, *Bos*, *Cervus*, and *Arvicola* occur in the loam, the latter in abundance. The most remarkable circumstance about these remains was the great excess of Deers' antlers above the others. Upwards of one thousand antlers, mostly shed and of young animals belonging chiefly

to *Cervus Guettardi*, were collected. The lower chamber was penetrated by Col. Wood, Dr. Falconer, and a friend last September, and found to have been washed out by the sea to a depth inwards of 31 feet; and at its extremity they met with a compact mass of marine sand and gravel, about 9 feet thick. The solid breccia forming the roof of the lower, and the base of the upper cave, increases in thickness from 6 feet at the outside to a greater depth inwards. Its materials correspond with the bed of angular *débris* observed by Mr. Prestwich on the raised beach of Mewslade Bay.

"Bowen's Parlour," or "Devil's Hole," is also a cavernous fissure in the limestone cliff, situated between Bosco's Den and Crow Hole. It has been washed out by the sea; portions only of its cave-deposits remaining, especially a diaphragm of cemented breccia, which divides the fissure into an upper and lower story; the former about 20 feet high at the mouth, the latter 14. Thin tabular aggregations of sand adhere to the lower surface of the partition, showing that it was deposited on a bed of sand. The same phenomena are repeated in "Crow Hole" with modifications, the cave-deposits being still *in situ*: here remains of *Ursus*, *Meles*, *Rhinoceros*, and some other forms have been found by Col. Wood.

"Raven's Cliff" presents a cavernous fissure broad and high externally, contracted within. Here a thin crust of stalagmite formed a floor upon sand 9 feet thick, which filled the fissure close up to the roof, leaving only an empty angular chamber about a foot high above the stalagmite. Upon the latter, remains of *Mustela foina*, *Canis vulpes*, and some Fish-bones and Bird-bones were found. In the sand large coprolites of Carnivores, some fine remains of *Felis spelæa*, bones of *Rhinoceros*, and the vertebræ of a Fish were discovered. Below the sand, as usual in the Gower Caves, there was a sandy breccia cemented by stalagmite, about a foot thick. Upon it a large block of limestone, smoothed and polished, probably by the rubbing of passing cave-animals, was discovered; and patches of polished surface were seen on the walls of the cave. Remains of *Elephas*, *Rhinoceros*, *Bos*, and *Cervus* were met with above the breccia. Below the breccia was a bed of dark-grey gritty sand, indurated by calcareous infiltration, and attaining a maximum thickness of about 8 feet. In this sand, and close upon the rock-floor, teeth of *Hippopotamus major*, young and old, and remains of *Ursus*, *Cervus*, and *Arvicola*, were met with. There was evidence, on the cliff beyond the aperture, of the cave and its contents having formerly been continued further seawards.

The author pointed out that in all these caves the bottom appears to have been first filled with sea-sand or shingle, with which were occasionally intermingled the bones of pachyderms, ruminants, &c., then living on the emerged land of Gower; that when this deposit was elevated above high-water mark, stalagmite and angular *débris* of limestone rock formed a floor, on which subsequently cave-earth or other common alluvial materials, with bones and antlers, often in profusion, were accumulated through the fissure above, during a long lapse of time after the rise had been accomplished. At last, by a converse action, of comparatively modern date, the level of the

caves was depressed. The raised beach at Mewslade Bay, which appears, according to the evidence of Mr. Prestwich, to be of later date than the Boulder-clay, has without doubt partaken of changes of level similar to what the caves and their contents have undergone, although, the marine deposits in the caves not being at a uniform level, either in relation to each other or to the raised beach, it is probable that there have been locally unequal depressions of level in comparatively modern times. The author thinks that the sea has effected but a comparatively slight inroad on the cave-deposits and raised beach; and hence he infers that they belong to a relatively modern epoch,—seeing also that they are probably of later date than the Boulder-clay period, and rest on marine sands containing existing species of shells.

Paviland Cave was next referred to; but the author restricted his remarks to the remains of *Elephas primigenius* and human bones that were found in it, and argues that the latter (*i. e.* the skeleton of the “Red Lady”) are of more recent date than the former.

In the cave at Spritsail Tor (cursorily examined by Sir H. De la Beche, and thoroughly explored by Colonel Wood), under a stalagmitic bone-breccia, the irregular fissure of the rocky floor was impacted with ochreous cave-earth full of bones and teeth of *Elephas antiquus*, *E. primigenius*, *Rhinoceros tichorhinus*, *Equus*, *Sus*, *Bos*, *Cervus*, *Lepus*, *Arvicola*, *Mus*, *Ursus spelæus*, *U. priscus*(?), *Felis spelæa*, *Hyæna spelæa*, *Canis lupus*, *C. vulpes*, *Meles taxus*, and *Mustela*. Coprolites of *Hyæna*, gnawed bones of *Bos*, *Equus*, and *Cervus*, and a great abundance of the detached molars of horse, gave the cave the undoubted character of having been a *Hyæna*’s den. In the superficial sand on the stalagmite, the antlers of a Reindeer and some human bones were found.

General remarks on the distribution of the Mammalian remains in the different caverns were offered, and the special anomalies pointed out; and, after a comparative review of the fauna of the Gower bone-caves in relation with that of other cave-districts of England in particular, and of Europe in general, the author arrived at the following conclusions as being consistent with the existing state of our knowledge :—

1. That the Gower Caves have probably been filled up with their mammalian remains since the deposition of the Boulder-clay.

2. That there are no mammalian remains found elsewhere in the ossiferous caves in England and Wales referable to a fauna of a more ancient geological date.

3. That *Elephas* (*Loxodon*) *meridionalis* and *Rhinoceros Etruscus*, which occur in, and are characteristic of, the “Submarine forest Bed” that immediately underlies the Boulder-clay on the Norfolk coast, have nowhere been met with in the British caverns.

4. That *Elephas antiquus* with *Rhinoceros hemitechus*, and *E. primigenius* with *Rh. tichorhinus*, though respectively characterizing the earlier and later portions of one period, were probably contemporary animals; and that they certainly were companions of the Cave-Bears, Cave-Lions, Cave-Hyænas, &c., and of some at least of the existing mammalia.

XXIX. *Intelligence and Miscellaneous Articles.*ON THE PERIODICITY OF THE SOLAR SPOTS, AND INDUCED
METEOROLOGICAL DISTURBANCES.*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

ASSUMING the mechanical energy exerted by the sun, as exhibited in its heat, light, and magnetism, to be directly created and sustained by the simplest yet grandest of the solar forces and motions, viz. by the force exerted by the planetary bodies (through gravity) acting upon the matter of the sun in rapid revolution around its axis (producing, if that matter be in a *solid* or coherent state, a violent and variable degree of tension, and if *fluid*, a violent agitation in eddies or tides),—would it not be reasonable to suppose that the orbital positions of the planets, especially at particular times, would not be without an effect as a disturbing element in the physical energy exerted by the sun itself?

It might even be natural to suppose that when the principal or larger mass of the planets are in (or nearly in) conjunction on one side of the sun in their orbits, such a disturbance would take place, owing to the centre of gravity of the solar system moving or shifting its position within the body of the sun itself, from an increased pull, or amount of attraction, exerted in one particular direction. May not some such disturbance be even at this moment taking place, and from this very cause? We have Venus, Jupiter, Saturn, and Uranus, all on one side of the sun, nearly in conjunction; and we have likewise seen during the whole of the present year, more or less, a great solar disturbance taking place, as proved by the unusual number of spots that have been visible.

Schwabe has proved that there is a spot-period, of about 11.1 years from *maximum* to *maximum*; and Col. Sabine, that this period coincides again with a particular class of magnetic cycles. It would indeed be a notable point gained to science, if the magnetic condition or force of the sun itself could be shown to depend upon, or possibly resolve itself into (as a correlated force), the force of gravitation itself! The most important thing then to ascertain is, whether or not a certain position of the planets in their orbits does not tend at certain times, through the force of gravitation, to create magnetical disturbance and increased mechanical energy in the sun? Jupiter, as far the largest planetary mass, would naturally have a predominating effect, and would tend to regulate the period of perturbation. Wolf, Carrington, and others have, I understand, already considered as more than a coincidence, the Jovian period of nearly 12 years, as connected with the spot-period of 11.1 years, without, however, arriving at any precise or definite results.

It would be desirable to ascertain how far the variations in the belts and markings on Jupiter himself agree with the spot-periods of the sun; it is more than possible that a mutual connexion might be traced.

Should the extremely cold, wet, and windy weather experienced

the last ten or eleven months in any way have arisen from solar magnetic disturbance, which there is some reason perhaps for supposing, and this, again, from the position of the planets above-named in conjunction, there is reason to fear there may not be much amelioration in our weather for some months to come, seeing that Venus, Jupiter, and Saturn will remain some time longer in much the same relative position.

M. Rudolph Wolf of Berne has shown that those years remarkable for abundance of solar spots have also been more than commonly rich in auroræ boreales. The great auroral display at the commencement of September 1859, occurring about the time for the return of sun-spot *maximum*, and which seems to have been visible over the greater portion of both hemispheres, appears to have been the precursor of a great meteorological disturbance: in England and Northern Europe more than an average amount of cold, wind, and rain have prevailed ever since; in North America and India more than an average amount of drought and heat. The opinions of philosophers differ respecting the influence of a paucity or an abundance of solar spots upon the temperature and seasons of the earth; the probability is, there is simply a general disturbance, arising from increase of (solar) magnetic influence, which may produce greater heat and dryness in one part of the globe, and more cold and rain in other parts.

I have merely hazarded these few remarks in the hope of drawing further attention to such interesting and important topics.

Yours respectfully,

Manchester, 23rd August, 1860.

ROBERT P. GREG.

ON THE PHENOMENA OF HEAT WHICH IN CERTAIN CASES ACCOMPANIES THE VIBRATORY MOTION OF BODIES. BY M. LE ROUX.

The author observed that, in attempting to create a nodal line in a body at a place at which it would not naturally be produced, there is a disengagement of heat at this place, this being the form under which the forces evoked by the vibratory motion manifest themselves.

The experiment may be made by clamping an elastic plate of any material at some distance from one end, and presenting the other to a toothed wheel. The heat produced is great enough to be perceptible to the hand.

The same fact may also be observed by soldering a small piece of brass to a steel spring, and then binding one end of the galvanometer to the steel and the other to the brass. When the spring is made to vibrate, the motion, which is obstructed at the place where the brass is, creates an elevation of temperature which produces an electric current.

It is important to compare this experiment with those of MM. Sullivan and Ermann*. In Le Roux's experiment the place of the soldering is not directly excited, and consequently all effects of friction are eliminated. If his results appear to confirm those

* De la Rive, *Traité d'Electricité*, vol. ii. p. 574.

obtained by these physicists, the author thinks that they do not confirm their explanation, according to which the vibration communicated to the soldering of two heterogeneous bodies *directly* produces an electric current. He thinks that the production of the heat precedes that of the electric current. It is seen, on examination, how the existence of the current is connected with the vibration—that the current ceases with the vibration, but not directly as if it had been interrupted by cutting the wire.

It appears natural to consider the electric effect observed in these experiments as a resultant of the heat disengaged at the surface of contact of two heterogeneous bodies which tend to enter into a common vibratory motion.—*Comptes Rendus*, vol. l. pp. 656, 729; *Bibliothèque Universelle*, vol. viii. p. 151.

ON THE DENSITY OF ICE. BY M. L. DUFOUR.

The question of the density of ice is certainly not settled. The data of different authors vary considerably, and even the latest researches have not led to very concordant results. In 1807, Placidus Henrich found this density to be 0.905; subsequently it was found by Thomson to be 0.94; Berzelius, 0.916; Dumas, 0.950; Osann, 0.927; Plücker and Geissler, 0.920; C. Brunner, 0.918; and lastly, H. Kopp, in 1855, found that it was 0.909. These divergencies, expressed in the increase of volume at the moment of congelation, correspond to values between $\frac{1}{9}$ and $\frac{1}{18}$.

In a series of researches on the congelation of water and of saline solutions, the author has had occasion to investigate the density of ice. The method which he has chosen, in order to avoid the serious difficulties incident to the use of the ordinary methods in the case of ice, consists in forming a liquid in which ice floats in equilibrium, and then determining the density of this liquid. The liquid was a mixture of water and alcohol, and all necessary precautions were taken to avoid, as much as possible, sources of error. Preliminary trials on bodies of known density showed that the method gave an approximate value which was not more than 0.002 from the true. The ice was quite free from air, and was obtained from distilled water boiled for some time. The method gave a ready means of determining the highest and lowest level of the density of each specimen.

The details of the research are published in the *Bibliothèque Universelle de Genève* for June 1860. For the majority of the specimens examined, 0.922 or 0.923 was certainly the highest, and 0.914 the lowest limit of the density. Twenty-two experiments gave a mean density of 0.9175 with a mean deviation of ± 0.0007 . The greatest deviations were $+ 0.002$ and $- 0.0013$.

The number 0.9175, which may be safely taken as giving the density of ice at 0° C., is almost exactly that of C. Brunner (0.9180), which was obtained by a different method. This corresponds to an increase of volume at the moment of congelation of $\frac{9}{1000}$, or very nearly $\frac{1}{11}$.—*Comptes Rendus*, June 4, 1860.

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[FOURTH SERIES.]

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XXX. *On the Electric Light of Mercury.*
By J. H. GLADSTONE, Esq., Ph.D., F.R.S.*

[With a Plate.]

PROFESSOR WAY has recently devised a plan by which a brilliant electric light is produced in an interrupted current of mercury. The metal is made to fall from a jet in a thin stream into a cup, the jet being half an inch or thereabouts above the surface of the metal in the cup. The upper reservoir and the lower vessel are in connexion with the two wires of a Bunsen's battery. As the galvanic force traverses the thin stream of mercury, it scatters and diffuses it in vapour with the production of a most intense light. For obvious reasons the gaseous mercury is confined in a well-closed glass cylinder; and this is made of sufficiently small diameter to become hot in the neighbourhood of the electric flame, and thus prevent the condensation of the mercury on it.

While inspecting this light about a year ago, I was struck by the strange manner in which it modified the apparent colours of surrounding objects, and especially with the ghastly purple and green hues which it imparted to the faces and hands of the spectators. This led me to a more careful examination and a prismatic analysis of the light itself. As these electrical flames are attracting much attention at the present moment, the observations then made may prove of some interest as a contribution to the general stock of facts.

Chevreul's "cercles chromatiques," when examined by this mercury light, showed very little, if any, red, but the violet was very luminous and distinct. Flowers, dyed wools, ribbons, &c.

* Communicated by the Author.

Phil. Mag. S. 4. Vol. 20. No. 133. Oct. 1860.

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were generally altered in colour ; but the following substances of known composition are more instructive in the contrast between the colours they then exhibited and those they display by daylight.

Crystals of protosulphate of iron appeared absolutely colourless instead of pale bluish green.

The blue sulphate of copper, and the yellow chromate of potash, were only enhanced in the brilliancy of their colours.

Bichromate of potash became of a yellow instead of a red-orange, and dull instead of bright.

Red prussiate of potash showed a yellow powder, and dingy-orange crystals.

Chloride of cobalt gave a dirty-brown instead of a pale-red solution.

Nitrate of chromium, though in such strong solution as to appear red by sunlight, was only dark dull green.

Amorphous phosphorus showed scarcely any red, or other colour, but presented a dark metallic appearance.

Iodide of mercury was not scarlet, but of a brown colour, very glistening, suggesting the idea of scales of tarnished silver.

Gold looked like brass.

Cochineal in aqueous solution appeared violet in a thin stratum, red brown in a deep one.

A decoction of coffee with milk, as usually drunk, assumed a dirty green hue.

A solution of bisulphate of quinine, uranium glass, and certain diamonds, exhibited the phenomena of fluorescence more beautifully even than by sunlight.

Among substances that had much the same colour by daylight and by the mercury flame, may be mentioned blue cobalt salts, yellow nitrate of uranium green chlorophyll, and purple aniline-dye, permanganate of potash and murexide.

On analysing this light by means of Prof. Powell's refraction goniometer, I found it to consist of a number of separate and variously coloured rays. This appearance is represented in Plate III. fig. 1, the angle of the prism being 45° . None of the lines are of any appreciable breadth ; that is, they are only as broad as the slit itself ; but the more intense ones appeared broader on account of irradiation, and of greater extent because the slightly luminous environment of the spark gives a perceptible amount of these rays. These peculiarities of the phenomenon as actually observed are retained in the figure, because they serve as a rough representation of the relative intensity of the different rays ; but this is more correctly indicated by the numbers affixed to the different lines : 1 denoting the brightest lines, or those of the first magnitude ; 2 the next brightest, and

so on. Fig. 2 represents the principal dark lines of the solar spectrum, as seen by means of the same prism, and is introduced to identify the positions of the luminous rays in fig. 1. The following Table gives the angular measurements of the more important rays, those of Fraunhofer's lines being appended in a separate Table:—

Colour.	Angular measurement.	Magnitude.
Red	31° 8	8
Orange	31 24	5
"	31 26.5	6
"	31 27	7
Yellow	31 32	4
"	31 35	1
"	31 35.5	1
"	31 46	1
"	31 49	4
Green	32 1	6
"	32 6	3
"	32 9	3
Blue	32 47	1
Violet	33 11	3
"	33 15	2
"	33 22	
"	33 31	
"	33 38	
"	34 10	

Letter.	Angular measurement.
A	31° 0
B	31 10
C	31 16
C6	31 23
D	31 32
E	31 54
b	31 59
F	32 13
G	32 51
G 33	33 11
H	33 23
K	33 27
I	33 36
N	34 7

Whether the thin stream of mercury was longer or shorter, whether the light was steady or intermittent, and whether it was more or less brilliant, the relative intensity of these luminous rays appeared the same, except as regards the most refrangible ray, which varied considerably in visibility. This ray is situated far beyond what is ordinarily considered the limit of the luminous spectrum, indeed at the utmost verge of the most refrangible light which I have been able to see with bright midsummer sunshine and special arrangements for detecting it. It is evident, therefore, that this ray exists in the electric light of mercury with an intensity vastly greater than it exhibits in the solar beam; and doubtless many objects (as cochineal) reflect or transmit a colour when examined by this light, with which, under other circumstances, we are unacquainted. It should be borne in mind, moreover, that this ray, as observed through the refraction goniometer, had passed through several pieces of glass—a medium which does not transmit freely the extra-violet rays. As to its colour, it seems to differ considerably according to the intensity; and the eye is perhaps not a good judge of a colour which, strictly speaking, it has never witnessed before. When bright, however, it may be best described perhaps as red-violet;

but when seen through cobalt glass it appeared reddish grey, or nearly colourless.

The prismatic analysis of this light explains at once the various chromatic phenomena mentioned above. The brilliancy of the yellow, blue, and violet rays accounts for the beautiful colour of those objects which can reflect these rays; while the feebleness of the red shows the reason of the great changes which red substances invariably undergo—sometimes merely to brown, at other times to purple, green, or whatever other colour in addition to red is principally reflected by them. Thus the blood, wherever it shows through the skin, as in the lips, appears of a bluish-purple instead of a reddish hue. And this explains also why sulphate of iron becomes perfectly colourless; for the ordinary green tint of this salt is due solely to its not transmitting with any freedom the red rays: it transmits all others, and therefore all those which prevail in the mercury-light. Hence it appears of the same colour as the source of illumination itself, which the eye ordinarily recognizes as white, though, as compared with the sun, it is decidedly coloured.

On searching afterwards for previous observations on the light of mercury, I found that Prof. Wheatstone, in his brief notice "On the Prismatic Decomposition of Electrical Light," in the Report of the British Association for 1835, mentions the spectrum of mercury as containing seven definite rays; but he does not further describe it. Masson*, in his elaborate paper, does not mention the light from mercury; neither does Robiquet†, nor Secchi‡. Angström§, however, gives a drawing of the lines that make their appearance in such a light. They agree closely with the more intense lines of my drawing, but with this important difference—that he has not figured any violet ray except the least-refrangible one. Thus he never saw the beautiful rays about and beyond H. In his investigation, the Swedish philosopher made the notable discovery that the spectrum of an electric spark is ordinarily composed of two distinct spectra—the one belonging to the gas across which the spark leaps, the other to the conducting metal or other body. By observing different metals in different gases, he was able to distinguish these two spectra; and he gives drawings of the lines due to oxygen, nitrogen, &c., by a comparison of which with my map I find that the mercury spectrum delineated by me is due solely to the gaseous metal, and that no air was traversed by the electric spark in Professor

* *Ann. de Chim. et de Phys.* 3rd series, vol. xxxi. p. 295.

† *Comptes Rendus*, October 31, 1859.

‡ *Nuovo Cimento*, vol. i. p. 405.

§ *Phil. Mag. S. 4.* vol. ix. p. 327.

Way's apparatus. Thus through his kindness I probably had the advantage of examining a purer as well as a far more brilliant mercurial light than any previous observer.

Attention is being particularly directed at the present time to the fact that the luminous bands of some artificial flames coincide exactly with dark lines in the solar spectrum, and that certain flames absorb light of the same refrangibility as they emit. With reference to the latter phenomenon, I have looked at the double spectrum of the sun shining through the mercury flame; but the rays from the metal were too intense to permit of any evidence of absorption of the solar beams in the same place. A glance at the preceding Table will show that a bright line occurs in the spark of mercury as in many other artificial lights just where D occurs in the solar spectrum; another ray appears to have the same refrangibility as the most prominent line between G and H, namely G 33: otherwise there seems to be no coincidence.

XXXI. *Further Researches regarding the Laws of Chromatic Dispersion.* By MUNGO PONTON, F.R.S.E.*

THROUGH the kindness of Dr. J. H. Gladstone, the author has been recently furnished with additional experimental data by which to test the laws developed in his former communication.

He has also revised his method of determining the relative values of the wave-lengths corresponding to the seven principal fixed lines of the spectrum, referred to that of B as unity.

Calling $E \div H = \rho$, $B \div E = \sigma$, $C \div E = \tau$, $E \div G = \nu$, $D \div E = \chi$, and $E \div F = \psi$, the relations subsisting among these six fractions, as fairly deducible from Fraunhofer's two sets of observations, may be expressed by the following six equations:—

$$(1) \quad \tau - \nu = 0.022'.$$

$$(2) \quad \rho = 60(\tau - \nu) = 1.33'.$$

$$(3) \quad 3(\chi - \psi) = 5(\tau - \nu) = 0.11'.$$

$$(4) \quad (\sigma + \chi) - (\tau + \nu) = 2(\tau - \nu) = 0.044'.$$

$$(5) \quad 2(\sigma - \chi) = 17(\tau - \nu) = 0.377'.$$

$$(6) \quad 2(\sigma + \tau + \nu + \chi) = 441(\tau - \nu) = 9.799'.$$

Hence the values of the fractions are—

* Communicated by the Author.

	Nos.	Logs.
ρ	$= 1.333333'$	0.1249387
σ	$= 1.308333'$	0.1167184
τ	$= 1.247222'$	0.0959439
ν	$= 1.225000'$	0.0881361
χ	$= 1.119444'$	0.0490025
ψ	$= 1.082407'$	0.0343906

The values of the normal wave-lengths thence arising are—

	Nos.	Logs.
B	$= 1.000000$	0.0000000
C	$= 0.953291$	$\bar{1}.9792255$
D	$= 0.855626$	$\bar{1}.9322841$
E	$= 0.764331$	$\bar{1}.8832816$
F	$= 0.706140$	$\bar{1}.8488910$
G	$= 0.623944$	$\bar{1}.7951455$
H	$= 0.573248$	$\bar{1}.7583429$

Fraunhofer's two sets of observed values referred to B as unity are—

	C	D	E	F	G	H
I.	0.954349	0.855962	0.764660	0.704053	0.623770	0.571035
II.	0.953168	0.855962	0.765447	0.706021	0.624557	0.576151
	-1181		$+787$	$+1968$	$+787$	$+5116$

The differences between the calculated and the observed values are—

C 2nd ob.	D	E 1st ob.	F 2nd ob.	G 1st ob.	H mean.
$+0.000123$	$+0.000030$	-0.000329	$+0.000119$	$+0.000174$	-0.000345

These latter differences are greatly less than those between the two sets of observations, and are so trifling as to be of no account.

Among the observations furnished by Dr. Gladstone are the following, on oil of cassia at temp. 13° C. Angle of prism $30^{\circ} 35' 20''$:—

Deviations.

A $18^{\circ} 58' 50''$, B $19^{\circ} 11' 10''$, C $19^{\circ} 18' 30''$, D $19^{\circ} 37'$, E $20^{\circ} 6' 10''$,
F $20^{\circ} 34' 30''$, G $21^{\circ} 36' 40''$.

Dr. Gladstone affirms that, owing to an absorptive action exerted by this medium on the violet end of the spectrum, the line H is *imperceptible*. The following are the corresponding indices of refraction :—

B 1.595416 , C 1.599083 , D 1.608243 , E 1.623057 , F 1.636980 , G 1.667829 .

The observer gives the foregoing as the means of several observations, stating that, while the individual observations sometimes varied as much as $1'$, he does not think that the mean values can differ much above $30''$ from the truth. A difference of $30''$ corresponds to one of 0.00035 on the index of B, and of 0.0003 on that of G.

In testing by these observations the general formula

$$\mu = \lambda^n \div \frac{\lambda^n}{\epsilon_n} - a_n,$$

it is needful, owing to the absence of H, to vary the method of procedure, and to supply the index of H by calculation. The exponent n must in this instance be found by trial. Then in determining ϵ_n and a_n we must make

$$(5B^n + 3C^n + D^n) - (5G^n + 3F^n + E^n) \\ \div \left(\frac{5B^n}{\mu_B} + \frac{3C^n}{\mu_C} + \frac{D^n}{\mu_D} \right) - \left(\frac{5G^n}{\mu_G} + \frac{3F^n}{\mu_F} + \frac{E^n}{\mu_E} \right) = \epsilon_n,$$

and

$$\frac{1}{6} \left\{ \frac{B^n + C^n + D^n + E^n + F^n + G^n}{\epsilon_n} - \left(\frac{B^n}{\mu_B} + \frac{C^n}{\mu_C} + \frac{D^n}{\mu_D} + \frac{E^n}{\mu_E} + \frac{F^n}{\mu_F} + \frac{G^n}{\mu_G} \right) \right\} = a_n.$$

With the exponent $n=3.1$ we obtain from these formulæ $\log \epsilon_n = 0.1972574$ and $a_n = 0.008186$.

The index of H calculated from the formula $\mu = \lambda^n \div \frac{\lambda^n}{\epsilon} - a_n$ is 1.697760 , and the differences between the other indices calculated from that formula and their observed values are—

B+	C—	D+	E—	F—	G—
0.000068	0.000261	0.000302	0.000002	0.000005	0.000130.

These differences all lie within the assigned limits of experimental error.

The extrusions or displacements of the fixed lines from their normal relative positions, are with the *calculated* indices as follow:—

b_x —	c_x —	d_x +	e_x +	f_x +	g_x —	h_x —
0.001675	0.000615	0.001103	0.001332	0.001624	0.000031	0.002238.

This series corresponds to the regular type, thus removing the anomaly presented by this medium, according to the observations of Professor Powell. Gladstone's observations were made on the same specimen of the oil of cassia redistilled; and his statement that the line H is imperceptible raises a strong probability that Professor Powell, in determining the index of that line, had been deceived. This probability is enhanced by

the circumstance that if Powell's index of H be rejected, and if his other indices be treated like Gladstone's, they exhibit the same close agreement with the exponential law, and in like manner conform to the regular type, as respects the laws of extrusion.

Thus treated, Powell's observations at temp. $22^{\circ}5\text{C.}$ give, with the exponent $n=3\cdot1$, the following values: $\log \epsilon_n=0\cdot1955608$, and $a_n=0\cdot008324$, whence arise these indices:—

B	C	D	E	F	G	H
1-589507	1-592902	1-602713	1-617366	1-631422	1-662470	1-692841

Excluding H, the differences between these and the observed indices are

B+	C-	D+	E-	F+	G-
0-000007	0-000098	0-000113	0-000034	0-000042	0-000030,

a result still more favourable than with Gladstone's observations.

Powell's observations at temp. 14°C. , yield with exponent $n=3\cdot4$, $\log \epsilon_n=0\cdot1979154$, and $a_n=0\cdot006895$; but a better equalization of errors is obtained by making $a_n=0\cdot006886$. Hence arise the following indices:—

B	C	D	E	F	G	H
1-594624	1-597724	1-606959	1-621213	1-635279	1-667343	1-699744.

Excluding H, the differences between these and the observed indices are

B+	C-	D-	E+	F-	G+
0-000124	0-000176	0-000341	0-000513	0-000521	0-000243.

Those at temp. 10°C. , yield with exponent $n=3$, $\log \epsilon_n=0\cdot1971516$, and $a=0\cdot008815$; but a better equalization of errors is obtained by making $a=0\cdot008840$. Hence arise the following indices:—

B	C	D	E	F	G	H
1-596758	1-600245	1-610314	1-625189	1-639332	1-670242	1-700156.

Excluding H, the differences between these and the observed indices are

B+	C-	D-	E+	F+	G+
0-000458	0-000455	0-000086	0-000289	0-000432	0-000442.

The differences in these two latter cases exceed those in the two former. Still, they are within the probable limits of error for single observations, and are but slightly in excess of those incident to mean observations.

In judging of these results, allowance must be made for probable variations of temperature or mechanical agitations occurring in the course of the experiments. A change of 1°C. will alter an index in this medium by about 0-001 on an average; so that a variation of a fraction of a degree, occurring while the

observations are in progress, may produce a considerable effect in augmenting or diminishing the apparent extrusions of the fixed lines, and in altering the relations of the indices to each other. In this manner we may account for the differences in the proportion of extrusion, and the consequent variations in the exponent, in the three observations by Powell, and for the better agreement of one set than another with the exponential law.

The extrusions for these three with the corrected indices, in the order temp. $22^{\circ}5$, temp. 14° , temp. 10° , are

b_x-	c_x-	d_x+	e_x+	f_x+	g_x-	h_x-
0.001703	0.000625	0.001123	0.001861	0.001652	0.000034	0.002274
1920	722	1248	2115	1901	7	2615
1622	591	1074	1768	1563	40	2152

These all conform to the regular type, so that the anomaly formerly presented by this medium may be now fairly regarded as quite removed.

From the four sets of observations on oil of cassia, may be determined the constant most suitable for ascertaining the exponent n with the improved normals. The most probable value of this constant appears to be 10.953160 , which is exactly twice the sum of the normal wave-lengths. Hence the formula for finding the exponent assumes the very simple form of $n=1+2S \times \frac{2X}{a}$, or $n=1+\frac{4SX}{a}$. Thus all the elements of calculation are linked together by determinate relations.

In a joint paper by Messrs. Dale and Gladstone in the Philosophical Transactions, 1858, there are given the following indices at temp. 15° C. for the bisulphide of carbon, the medium next in importance to the oil of cassia:—

B	C	D	E	F	G	H
1.6177	1.6209	1.6303	1.6434	1.6554	1.6799	1.7035.

These numbers give $n=2.6$, $\log \epsilon_n=0.2022216$, $a_n=0.009561$. The differences between the indices calculated from these elements and the observed indices are

B-	C+	D-	E-	F-	G+	H-
0.000040	0.000081	0.000036	0.000037	0.000080	0.000360	0.000240

These differences all lie within the recognized limits of experimental error.

The extrusions arising from the *rectified* indices are

\bar{b}_x-	\bar{c}_x-	\bar{d}_x+	\bar{e}_x+	\bar{f}_x+	\bar{g}_x-	\bar{h}_x-
0.001091	0.000385	0.000736	0.001178	0.001021	0.000049	0.001410.

Before the rectification of the indices the extrusions are slightly irregular; but the above conform to the regular type.

It is a delicate operation so to adjust the normals as to pro-

duce the least amount of error in these two media—the oil of cassia and the bisulphide of carbon—and to render both regular in their extrusions while at the same time preserving definite relations among the normals themselves. Those adopted in the foregoing calculations fulfil all these conditions, of which one or other would be violated by even a comparatively small departure from the values above assigned. Thus the exponential law, combined with the laws of extrusion, while first detected by means of an assumed set of normals, have in their turn become the means of discovering the best values of the normals themselves. With media of lower refractive and dispersive powers, any inaccuracy in the normals is of less moment, and might easily escape detection.

For hydrate of phenyle, temp. 13° C., Messrs. Dale and Gladstone's indices are

B	C	D	E	F	G	H
1.5416	1.5433	1.5488	1.5564	1.5639	1.5763	1.5886,

whence $n=2.34$, $\log \epsilon_n=0.1831359$, and $a_n=0.007193$; but $a_n=0.007215$ gives a better equalization of errors.

The differences between the calculated and observed indices are

B-	C+	D+	E+	F-	G+	H+
0.000115	0.000219	0.000270	0.000244	0.000552	0.000537	0.000182.

The observed indices being the results of a single observation, these differences all lie within the limits of probable error. Before correction of the indices, the extrusions are slightly irregular; but with the corrected indices they become regular, and are as follow:—

b_x-	c_x-	d_x+	e_x+	f_x+	g_x-	h_x-
0.000579	0.000199	0.000395	0.000621	0.000531	0.000036	0.000733.

For anhydrous ether at temp. 15° C., Messrs. Dale and Gladstone give these indices,

B	C	D	E	F	G	H
1.3545	1.3554	1.3566	1.3590	1.3606	1.3646	1.3683,

whence $n=2.73$, $\log \epsilon_n=0.1306117$, and $a_n=0.002072$. The differences between the calculated and observed indices are

B+	C-	D+	E-	F+	G+	H+
0.000156	0.000214	0.000076	0.000214	0.000111	0.000113	0.000058.

With the *corrected* indices the extrusions are

b_x-	c_x-	d_x+	e_x+	f_x+	g_x-	h_x-
0.000278	0.000099	0.000187	0.000230	0.000263	0.000012	0.000361.

For absolute alcohol, temp. 15° C., their indices are

B	C	D	E	F	G	H
1.3612	1.3621	1.3638	1.3661	1.3683	1.3720	1.3751,

whence $n=1.9$, $\log \epsilon_n=0.1316156$, and $a_n=0.003955$. The differences between the calculated and observed indices are

B+	C-	D+	E+	F-	G-	H+
0.000080	0.000122	0.000012	0.000081	0.000118	0.000006	0.000086

and with the corrected indices the extrusions are

b_x-	c_x-	d_x+	e_x+	f_x+	g_x-	h_x-
0.000154	0.000051	0.000107	0.000163	0.000137	0.000013	0.000189.

For distilled water, temp. 15°C. , their indices are

B	C	D	E	F	G	H
1.3300	1.3307	1.3324	1.3347	1.3366	1.3402	1.3431,

giving $n=1.87$, $\log \epsilon_n=0.1215388$, and $a_n=0.004023$, or to equalize errors, $a_n=0.004018$. The differences between the calculated and observed indices are

B+	C-	D+	E-	F-	G-	H+
0.000005	0.000030	0.000016	0.000036	0.000046	0.000054	0.000048;

and with the corrected indices the extrusions are

b_x-	c_x-	d_x+	e_x+	f_x+	g_x-	h_x-
0.000148	0.000049	0.000102	0.000158	0.000131	0.000012	0.000182.

In these three last cases, the differences between the calculated and observed indices all lie within the limits of probable error, and with the corrected indices the extrusions are all regular.

The results deduced from Messrs. Dale and Gladstone's observations on water are of the first class, and agree closely with those deduced from Fraunhofer's two sets of observations. The large discrepancies presented by Powell's observations on this medium are thus shown to be entirely caused by experimental errors.

These results all tend to establish the perfect universality of the laws developed in the author's former paper.

For the six media above discussed, Messrs. Dale and Gladstone have given the index of the fixed line A. Fraunhofer left no record of the normal wave-length corresponding to this line. By comparing together Messrs. Dale and Gladstone's indices, while keeping in view the likelihood that this normal bears some definite relation to the others, the conclusion has been reached that its most probable value in reference to B as unity is 1.121019 , $\log 0.049613$. This makes the value of the fraction $A \div E = 1.466'$, which is $= 66 (\tau - \nu)$, thus establishing a definite relation between this and the other normals.

This value of A being assumed, we have the following discrepancies between the calculated and observed indices of this line for the six media :—

	Observed.	Differences.
Oil of cassia . . .	1.589241	+0.000054
Bisulphide of carbon.	1.6114	-0.000145
Hydrate of phenyle .	1.5377	-0.000195
Water	1.3284	+0.000241
Alcohol	1.36	-0.000148
Anhydrous ether . .	1.3529	+0.000748

These discrepancies, excepting the last, are quite moderate, lying within the limits of probable error for *mean* observations; while even the large discrepancy in the case of ether is within the probable limits of error for single observations. The indices for oil of cassia and bisulphide of carbon are those from which the best judgment may be formed; because any alteration of the normal tells more on these indices than on any of the others. The close agreement with observation in these two cases shows that the above value of the normal wave-length corresponding to the line A must be very nearly exact.

It may be interesting to show the effect of introducing A into the account in the process for correcting the indices. Take, for example, the bisulphide of carbon. Assuming the same exponent as before, $n=2.6$, we obtain $\log \epsilon_n=0.2022496$, and $a_n=0.009543$. The indices thence arising differ very slightly from those obtained without taking A into account. The distribution of errors is somewhat different. The following are the differences between the calculated and observed values:—

A—	B+	C+	D	E—	F—
0.000074	0.000017	0.000132	0.000000	0.000026	0.000086
	G+	H—			
	0.000302	0.000351.			

Seeing that the greater the number of observations on which the calculation is based the more likely are the individual indices to be rendered accurate, the above are probably more correct than those previously obtained without taking A into account.

It may accordingly be well, in all future observations, to take the deviation of A, as well as those of the other seven principal lines, and base the calculation on the whole eight. This, however, is rather practically expedient than theoretically needful. Had we given only two extreme indices and one central, such as μA , μD , μH , and could we rely implicitly on their perfect accuracy, it would be easy from these to determine all the other five. It is only because of the imperfection of the present modes of observation, and the consequent impossibility of obtaining absolutely accurate indices for these three lines, that it becomes expedient to take observations on all the eight indices into account, the better to secure a mutual compensation of experi-

mental errors; for an inaccuracy in the determination of the three assumed as the basis of calculation may be of such a character as greatly to increase the discrepancies between the observed and calculated indices of the other lines, and even to make them appear to exceed the legitimate bounds of probable error. Cases, however, may occur in which it may be necessary to rest contented with determining the other five indices from three being given.

To illustrate this point, and to exhibit the method of procedure in such a case, suppose that for the bisulphide of carbon we have given only ${}^{\mu}A$, ${}^{\mu}D$, and ${}^{\mu}H$, and let the *corrected* indices be assumed as those given. The first step is to find the exponent n . Call $A^n \div {}^{\mu}A = a_n$, $D^n \div {}^{\mu}D = d_n$, and $H^n \div {}^{\mu}H = h_n$. Make $(A^n - D^n) \div (D^n - H^n) = \Theta$ and $(a_n - d_n) \div (d_n - h_n) = \theta$, these two quantities, Θ and θ , being either integral or fractional. When the proper exponent is correctly found, then is $\Theta = \theta$. If the exponent be too low, Θ is the greater; but if the exponent be too high, θ is the greater. Now, although the difference between Θ and θ does not vary rateably with the difference between two exponents when the latter are widely apart, yet, if the difference between the two exponents do not exceed 0.5, the corresponding variations in the value of $\Theta - \theta$, or $\theta - \Theta$, are so nearly rateable as to suffice for the purpose of calculation. Hence if two exponents be taken, one too high, the other too low, and differing by not more than 0.5, the true exponent may be easily found.

If the medium be highly dispersive, like the oil of cassia, the two exponents may be 3 and 3.5. If the dispersive power be somewhat less, as in bisulphide of carbon, take 2.5 and 3, and for lower dispersive powers 2 and 2.5, 1.5 and 2, 1 and 1.5, according to a scale which will be easily found by practice. Then by dividing by 5 the variation in the value of the difference between Θ and θ , we obtain nearly the rate for each 0.1 of exponent, and can thence arrive at the true exponent to within 0.1, or even nearer if required.

Thus, in the case of bisulphide of carbon, with exponent 2.5 we have $\Theta = 1.525176$, and $\theta = 1.523900$, whence $\Theta - \theta = 0.001276$. With exponent 3 we have $\Theta = 1.786120$ and $\theta = 1.791053$, whence $\theta - \Theta = 0.004933$. Adding these two, we obtain 0.006209 as the total variation, corresponding to a difference in the exponent of 0.5; and one-fifth of this amount, or 0.001242, is the variation for each 0.1 of exponent. It is thus seen that a rise of 0.1 from 2.5 will make Θ so nearly equal to θ , that 2.6 may be regarded as the true exponent.

This quantity found, it is easy to determine $\log \epsilon_n$ and a_n .

For we have $(A^n - H^n) \div (a_n - h_n) = \epsilon_n$, and

$$\frac{1}{3} \left\{ \frac{A^n + D^n - H^n}{\epsilon_n} - (a_n + d_n + h_n) \right\} = a_n,$$

whence all the other indices may be found from $\mu = \lambda^n \div \frac{\lambda_n}{\epsilon_n} - a_n$.

These will be the same as those deduced from all the eight observed indices.

Take now the *observed*, instead of the *corrected* indices "A," "D," "H." With exponent $n=3$, we have $\theta - \Theta = 0.004642$; with $n=2.5$ we have $\Theta - \theta = 0.001587$, sum = 0.006229, one-fifth whereof, or 0.001246, is the rate for each 0.1 of exponent, being nearly the same as before. This makes the true exponent more nearly 2.62; but 2.6 is sufficiently near for calculation. Hence we have $\log \epsilon_n = 0.022553$, instead of, as formerly, 0.022496, and $a_n = 0.009553$ instead of 0.009543. The differences between the calculated and observed indices thence arising are

A—	B+	C+	D+	E+	F—
0.000033	0.000099	0.000185	0.000063	0.000050	0.000002
	G+	H—			
	0.000427	0.000326.			

These results, though probably less accurate than those obtained by using the whole eight observed indices for mutual correction, yet differ so little from them as to show that, except where great accuracy is required, the foregoing abbreviated process may be found practically useful. Indeed it may generally be trusted, at least to the extent of ascertaining the exponent.

When all the eight indices are taken into account, the aspect presented by the extrusions is wholly changed. The upper node is shifted from between C and D to between B and C, so that the transfer of motive energy appears to be from A, B, G, H to C, D, E, F. The following are the extrusions arising with the corrected indices, in the case of bisulphide of carbon:—

$a_x -$	$b_x -$	$c_x +$	$d_x +$	$e_x +$	$f_x +$
0.001837	0.000061	0.000481	0.001255	0.001374	0.001012
	$g_x -$	$h_x -$			
	0.000344	0.001880.			

The mutual relations of these are exhibited in this formula,

$$(7a_x + 5b_x) - (3c_x + d_x) = (7h_x + 5g_x) - (3f_x + e_x);$$

also calling $h_x - a_x = \delta_1$, $g_x - b_x = \delta_2$, $f_x - c_x = \delta_3$, and $e_x - d_x = \delta_4$, then $\delta_4 - \delta_1$, $\delta_2 - \delta_4$, and $\delta_3 - \delta_2$ form an arithmetical progression. In the above example the conformity to these two laws is not quite exact; but this arises merely from decimal imperfections.

The above extrusions probably represent the actual displacements of the fixed lines, arising from that property of the

medium which produces the irrationality, more correctly than do the extrusions arising when only seven lines are taken into the account. But this point cannot be satisfactorily ascertained until recourse be had to such a method of experiment as shall exhibit the irrationality apart from the dispersion.

Indeed it is manifest that, for the further elucidation of the laws of refraction and dispersion, it is needful to resort to a totally different method of experiment from what has been hitherto pursued, and to adopt such arrangements as shall present the effects of the refractive, the dispersive, and the extrusive powers of the medium, each separately and distinctly. The variations of each power, produced by change of temperature, may then be studied with some reasonable prospect of arriving at precise and satisfactory results.

XXXII. *On a new Self-registering Mercury Barometer.*

By THOMAS TATE, *Esq.**

[With a Plate.]

THE advantage of this instrument consists in the extent of its range as compared with that of the common barometer, and in its adaptation for the purpose of self-registration. It differs from the common barometer in having a floating cup, which rises or falls according as the column of mercury, balancing the pressure of the atmosphere, falls or rises, thereby producing an increased variation in the level of the mercury in the tube for any given change in the atmospheric column. The construction of the instrument is shown in Plate III. fig. 3, L K D is the barometer-tube fixed to the frame A B, having its lower or open extremity, D, inserted in the mercury, I F, contained in the glass float E G F; Q N a glass jar containing water slightly acidulated with sulphuric acid to arrest the evaporation from its surface. The glass float consists of two tubular portions, E M, open at the top, and I F closed at the bottom, and a globe G to give a force of floatation sufficient to balance the gravity of the mercury contained in the tubular portion I F; a light cap, with projecting arms *a* and *b*, is attached to the top of the tubular portion M E, to keep the float in position and to give motion to the indices of registration; *b* is a small weight touching the upper face of one of the projecting arms, and balancing the index weight *d* by means of a fine thread or hair passing over a wheel *e*; in like manner *a* is a small weight whose upper surface touches the under face of the other projecting arm; the index *d* registers the maximum pressure of the atmosphere, and *c*

* Communicated by the Author.

that of the minimum pressure. A slit is made in the board A E, covering the upper portion of the frame, to allow the tube E M freely to ascend or descend, as the case may be. C, c, and d are scales for reading off the height of the mercury column balancing the pressure of the atmosphere at any time. The upper portion, K L, of the tube D L has a greater section than the lower portion, and the section of the tube I F at the part I is greater than that of the lower portion. The action of the instrument is as follows.

If I C be the column of mercury in the tube corresponding to mean atmospheric pressure, then the instrument is so adjusted that the lower extremity, D, of the tube shall be at, or near to, the middle of the mercury cup I F, and that the liquid in the jar shall stand at, or near to, H, the middle of the tube E M. In this position (as well as in all other positions) of the instrument, the weight of the displaced liquid will be equal to the weight of the glass float added to the weight of mercury equal in volume to the space occupied by the mercury in the cup.

Now supposing an augmentation to take place in the atmospheric pressure, then the mercury will rise in the tube I C; and a portion of mercury being taken from the cup I F, the float will rise; and this will produce a further rise of the mercury in the tube; and this will go on until the loss of the force of floatation is equal to the weight lost by the mercury in the cup. On the other hand, when a reduction takes place in the atmospheric pressure, the column I C will fall, and the mercury in the cup being augmented, the float will sink, which will cause a further fall in the column in the tube; and this will go on until the increase of the force of floatation is balanced by the gravity of the mercury added to the cup. Hence it follows that the rise or fall, as the case may be, of the mercury in the tube D L will be greater than that which would be simply due to the change in the height of the ordinary barometric column.

The scales C, c, d are uniform in their graduations when the external sections of the tubes E M and D G are uniform, together with the internal section of the tube K L. These scales are graduated in the following manner:—

The actual height of the column I C is observed, and corresponding marks are placed upon the different lines on which the scales are to be formed; after a change has taken place in the atmospheric pressure, the height of the mercury column in the tube I L is again observed; and corresponding marks being made on the different lines of scales as before, the intervals between these marks will have the same reading as the difference between the two columns of observation: the scales may then be extended according to the readings of these intervals.

It must be observed that this increase of range is effected by the action of two independent forces, viz. the force of floatation of the cup, and the gravity of the mercury in the tube ; whereas in the common wheel barometer, the change of barometric column is simply multiplied by mechanical transmission.

Let P = the height of the mercury column I C measuring the atmospheric pressure.

v = the corresponding volume of the liquid displaced by the float, v' being put for the volume corresponding to p pressure.

M = the corresponding volume occupied by the mercury in the float.

A = the internal section of the jar H N.

a = the external section of the tube E M.

D = the diameter of the globe G.

b = the internal section of the wide portion of the tube I.

c = the external section of the tube D K.

k = the internal section of the tube K L.

e = the descent of the float corresponding to p pressure of the atmosphere.

x = the corresponding rise of the mercury in the cup I.

E = the corresponding fall of the mercury in the tube K L.

S = the specific gravity of the mercury, s being that of the liquid.

Then, when the float descends e inches, we find—

Increase of volume of liquid displaced, $v' - v = ea \cdot \frac{A}{A - a}$;

Incr. vol. mercury occupied by the mercury in the cup $= bx$;

$$\therefore Sbx = sea \cdot \frac{A}{A - a};$$

$$\therefore x = \frac{s}{S} \cdot \frac{ea}{b} \cdot \frac{A}{A - a}.$$

Vol. mercury added to the cup $= ec + (b - c)x$;

Vol. mercury displaced from the tube K L $= Ek$;

$$\therefore Ek = ec + (b - c)x;$$

$$\therefore E = \frac{e}{k} \left\{ c + \frac{sa}{Sb} \cdot \frac{A}{A - a} (b - c) \right\}.$$

But $P - p = E - e + x$;

$$\therefore \frac{e}{P - p} = r = \frac{k}{c - k + \frac{sa}{Sb} \cdot \frac{A}{A - a} (b + k - c)}, \quad \dots (1)$$

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and

$$\frac{E}{P-p} = R = \frac{r}{k} \left\{ c + \frac{sa}{Sb} \cdot \frac{A}{A-a} (b-c) \right\}. \quad (2)$$

If $b=a$, then equation (1) becomes

$$r = \frac{k}{c-k + \frac{s}{S} \cdot \frac{A}{A-a} (a+k-c)}. \quad (3)$$

Now as e , in equations (1) and (3), is proportional to $P-p$, it follows, the tubes being uniform in section, that the graduation of the scale d or c must be uniform; and equation (2) shows that the same observation applies to the scale C . Equations (1) and (3) express the range r of the instrument, estimated on the scale d or c , compared with that of the ordinary barometer; and similarly, equation (2) expresses the ratio of the range estimated on the scale C .

If $k=c$, that is, if the tube DG is selected so that it shall exactly pass within the tube KL , then equation (1) becomes

$$r = \frac{Sk}{sa} \cdot \frac{A-a}{A}. \quad (4)$$

Assuming b to be large as compared with k , then we get approximately,

$$E = e + P - p, \text{ and } \therefore R = r + 1; \quad (5)$$

that is to say, the ratio of range of the scale C exceeds that of the scale d or c by unity

In the instrument which I have constructed, $k = \cdot 08$, $c = \cdot 04$, $a = 1$, $\frac{s}{S} = \frac{1}{13\cdot 5}$, $A = 16$; then from equation (3) we get

$$r = \frac{\cdot 08}{-\cdot 04 + \frac{1}{13\cdot 5} \times \frac{16}{15} (1 + \cdot 04)} = 2 \text{ nearly.}$$

In this case the range is double that of the ordinary barometer.

And from equation (2) we get $R = 2\cdot 9$, or 3 nearly. In this case the range is three times that of the ordinary barometer.

Neglecting the weight of the float and the force of floatation due to the displacement of the tubular portions of the float, we find

$$D = \sqrt[3]{\frac{S}{s} \cdot \frac{M}{\cdot 5236}}. \quad (6)$$

This value of D is sufficiently exact for construction, inasmuch as a little more or less mercury in the tube IF would correct any slight error arising from the elements neglected. Thus if

$M=1.57$, then $D=3.4$ nearly, and vol. globe $=21$ cubic inches. Assuming $a=1$, $MH=3$, vol. tube $IF=1$, then

$$v=21+3 \times 1+1=25 \text{ cubic inches.}$$

As regards the disturbances arising from change of temperature, it is to be observed that the instrument, under all eligible forms of construction, is to a great extent self-corrective as regards the scale d or c ; for whilst the gravity of the float, by an augmentation of heat, is increased by the expansion of the liquid, at the same time this gravity is decreased by the expansion of the mercury which causes a portion of it to pass from the cup into the tube, it being borne in mind that the tube is wider at KL than it is at KD . But a certain value may be assigned to a and DK which shall render this correction strictly true for mean atmospheric pressure, and approximately true for all other pressures.

In order that the float may remain stationary under a change of temperature, the increment or decrement, as the case may be, of the force of floatation must be equal to the increment or decrement of the weight of mercury in the cup. Keeping this condition in view, let

S, s_1 = the weight of a cubic inch of mercury and liquid respectively at t_1 temperature, S and s being their respective values at t temperature.

v = the volume of displaced liquid at mean temperature t , and mean atmospheric pressure P .

c_1 = internal section of the small tube DK .

$\frac{1}{\alpha}, \frac{1}{\beta}, \frac{1}{\rho}$ = the expansion per unit of volume for mercury, water, and glass respectively, corresponding to t_1-t degrees of temperature.

$m = KC$, the column of mercury in the wide portion of the tube.

$m' = ID$, the length of immersion of the tube in the mercury.

$h = DK$, the column in the small portion of the tube.

h' = the length of the tube DK , measured from its fixed point of attachment K to the frame.

H = the height of the liquid in the glass jar.

Then we find—

Vol. mercury in the tubes DK and KC before expansion
 $=c_1 h + km$.

Vol. of this mercury after its expansion

$$=(c_1 h + km) \left(1 + \frac{1}{\alpha}\right) \dots (v').$$

Rise of atmospheric column due to change of density of the

$$\text{mercury} = P \frac{1}{\alpha}.$$

Vol. mercury in the tubes taking the expansion of glass and mercury into account

$$\begin{aligned} &= c_1 \left(1 + \frac{2}{3\rho}\right) h + k \left(1 + \frac{2}{3\rho}\right) \left(m + P \frac{1}{\alpha}\right) \\ &= \left(1 + \frac{2}{3\rho}\right) \left\{ c_1 h + k \left(m + P \frac{1}{\alpha}\right) \right\} \dots (v''). \end{aligned}$$

Increase of vol. of the mercury in the cup due to the elongation of the tube $DK = (c - c_1) \frac{h'}{3\rho} \dots (v''')$.

Now assuming that b is large as compared with k , and therefore that the level of mercury in the cup is not affected by the expansion of the mercury which is in it, we have

Vol. mercury taken from the cup $= v'' - v' - v'''$

$$= \left\{ (P - m)k - c_1 h \right\} \frac{1}{\alpha} + \left\{ 2(c_1 h + km) - (c - c_1)h' \right\} \frac{1}{3\rho}.$$

Neglecting $(c - c_1)h'$ as being small compared with $2(c_1 h + km)$, and taking $\frac{b}{b - c} = 1$, which it is in all cases very nearly, we get

Weight lost by the mercury in the cup,

$$w'' = \frac{S_1}{\alpha} \left\{ (P - m)k - c_1 h + \frac{2}{3}(c_1 h + km) \frac{\alpha}{\rho} \right\}.$$

$$\text{But } S_1 = \frac{S}{1 + \frac{1}{\alpha}}, \quad h - m' + m = P;$$

$$\therefore P - m = h - m', \text{ and } m = P + m' - h;$$

$$\therefore w'' = \frac{S}{1 + \frac{1}{\alpha}} \left\{ (k - c_1) \left(1 - \frac{2\alpha}{3\rho}\right) h + (P + m')k \frac{2\alpha}{3\rho} - m'k \right\}. \quad (7)$$

Again, to find the changes produced by the expansion of the liquid, &c., let z be put for the increment of height of the liquid due to the change of temperature; then we get

Vol. liquid before expansion $= AH - v$,

$$\therefore \text{Vol. liquid after expansion} = (AH - v) \left(1 + \frac{1}{\beta}\right).$$

But taking the expansion of the glass into account, we have also

$$\begin{aligned} \text{Vol. liquid after expansion} &= A \left(1 + \frac{2}{3\rho}\right) (H + z) - v \left(1 + \frac{1}{\rho}\right) \\ &\quad - a \left(1 + \frac{2}{3\rho}\right) z. \end{aligned}$$

By equating these two expressions, we get

$$z = \frac{(AH-v)\frac{1}{\beta} + v\frac{1}{\rho} - AH\frac{2}{3\rho}}{(\Lambda-a)\left(1 + \frac{2}{3\rho}\right)}.$$

$$\text{Buoyancy float after expansion} = s_1 v \left(1 + \frac{1}{\rho}\right) + s_1 a \left(1 + \frac{2}{3\rho}\right) z;$$

$$\therefore \text{loss of buoyancy of float, } w' = sv - s_1 v \left(1 + \frac{1}{\rho}\right) - s_1 a \left(1 + \frac{2}{3\rho}\right) z.$$

$$\text{Substituting the value of } z, \text{ and observing that } s_1 = \frac{s}{1 + \frac{1}{\beta}},$$

we get

$$w' = \frac{\Lambda}{\Lambda-a} \cdot \frac{s}{1+\beta} \left\{ v \left(1 - \frac{\beta}{\rho}\right) - Ha \left(1 - \frac{2\beta}{3\rho}\right) \right\}. \quad (8)$$

Now in order that the float may remain stationary under this change of temperature, w' must be equal to w'' ; hence we find by equating,

$$h = \frac{\frac{\Lambda}{\Lambda-a} \cdot \frac{s}{S} \cdot \frac{1+\alpha}{1+\beta} \left\{ v \left(1 - \frac{\beta}{\rho}\right) - Ha \left(1 - \frac{2\beta}{3\rho}\right) \right\} + \left\{ m' - (P + m') \frac{2\alpha}{3\rho} \right\} k}{(k-c_1) \left(1 - \frac{2\alpha}{3\rho}\right)}, \quad (9)$$

which gives the length of the small tube DK, so as to render the instrument self-corrective at mean atmospheric pressure, as regards the disturbances arising from change of temperature.

Now the relative values of a and v may be determined so as to render this principle of compensation applicable to all other atmospheric pressures.

Let the pressure of the atmosphere become P_1 , and let the rise of the float under this change of pressure be e_1 ; moreover, let v_1 = the volume of liquid displaced by the float, H_1 = the column of liquid in the jar, m'_1 = the length of immersion of the tube DK in the mercury; then

$$P_1 = P + \frac{e_1}{r}; \quad v_1 = v - ae_1 \cdot \frac{\Lambda}{\Lambda-a}; \quad H_1 = H - \frac{ae_1}{\Lambda-a};$$

and when $b = a$,

$$m'_1 = m' + e_1 - \frac{s}{S} \cdot \frac{\Lambda}{\Lambda-a} e_1 = m' + \left(1 - \frac{s}{S} \cdot \frac{\Lambda}{\Lambda-a}\right) e_1.$$

Substituting these values in equations (7) and (8), we get

$$w_1'' = w'' - \frac{S}{1+\alpha} \cdot k \left\{ \left(1 - \frac{2\alpha}{3\rho}\right) \left(1 - \frac{s}{S} \cdot \frac{\Lambda}{\Lambda-a}\right) - \frac{1}{r} \cdot \frac{2\alpha}{3\rho} \right\} e, \quad (10)$$

$$w_1' = w' - \frac{\Lambda}{\Lambda-a} \cdot \frac{s}{1+\beta} \cdot a \left\{ \frac{\Lambda}{\Lambda-a} \left(1 - \frac{\beta}{\rho}\right) - \frac{a}{\Lambda-a} \left(1 - \frac{2\beta}{3\rho}\right) \right\} e. \quad (11)$$

Now in order that the float may remain stationary under this change of temperature, we must have $w_1'' = w_1'$; but $w'' = w'$; hence we obtain by equating (10) and (11), and putting

$$\frac{\Lambda}{\Lambda-a} = \frac{n}{n-1}, \text{ or } \Lambda = na,$$

$$\begin{aligned} & \frac{S}{1+\alpha} \cdot k \left\{ \left(1 - \frac{2\alpha}{3\rho}\right) \left(1 - \frac{s}{S} \cdot \frac{n}{n-1}\right) - \frac{1}{r} \cdot \frac{2\alpha}{3\rho} \right\} \\ &= \frac{n}{n-1} \cdot \frac{s}{1+\beta} \cdot a \left\{ \frac{n}{n-1} \left(1 - \frac{\beta}{\rho}\right) - \frac{1}{n-1} \left(1 - \frac{2\beta}{3\rho}\right) \right\}. \end{aligned}$$

Eliminating r by means of equation (3), observing that $\frac{1+\alpha}{1+\beta} = \frac{\alpha}{\beta}$ very nearly, and solving the resulting equality for the value of a , we get

$$a = \frac{\left(3k \frac{\rho}{\alpha} - 2c\right) \left(\frac{n-1}{n} \cdot \frac{S}{s} - 1\right) (n-1)}{3(n-1) \frac{\rho}{\beta} - n}, \quad (12)$$

which gives the value of a in terms of k , c , n , &c., in order to render the instrument self-corrective for changes of temperature under all atmospheric pressures. No correction, therefore, being required for the indications of the scale d or c , that of C will require the same correction as in the ordinary barometer.

The rates of expansion of the substances being uniform, this expression is independent of $t_1 - t$, that is, it holds true for all temperatures. The same observation applies to equation (9).

Taking $t_1 - t = 18^\circ$, then $\frac{1}{\alpha} = \frac{1}{550}$, $\frac{1}{\rho} = \frac{1}{4160}$; and calculating the mean rate of the expansion of water from the experiments of Hallström between the temperatures of 46° and 86° , we find $\frac{1}{\beta} = \frac{1}{560} = \frac{1}{\alpha}$ nearly. Also let $k = .08$, $c = .04$, $\frac{S}{s} = 13.5$, $n = 16$.

Then from equation (12) we find $a = .94$.

From equation (9), taking $c_1 = .026$, $H = 17$, $P = 29.5$, $m' = 4$, and $v = 25$, we find $h = 10.96$, or 11 inches nearly.

The values of a and h here found, form the conditions of compensation for the disturbances arising from change of temperature.

From equation (3), we find

$$r = \frac{.08}{-.04 + \frac{1}{13.5} \times \frac{16}{15} (.94 + .04)} = 2.16.$$

From equation (2), taking $b = a$, we find $R = 2.94$, or 3 nearly.
Hastings, September 4, 1860.

XXXIII. On the Periodicity of the Solar Spots.

By R. P. GREG, F.G.S.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

IN the last Number of this Journal, while briefly suggesting that the recent meteorological disturbances we have experienced might be connected with increased magnetic action in the sun, as apparent in the phenomenon of periodicity in spot-maximum, I at the same time alluded to the possibility of these periodical changes on the sun's surface being likewise a result, more remotely, of planetary influence. I stated that Mr. Carrington and M. Wolf had likewise entertained the same idea, though without, I believed, arriving at any precise or satisfactory results. In the *Comptes Rendus* for January 1859, M. Wolf gives a formula which, according to the Rev. Robert Walker*, "expresses the number of years from the mean frequency of the spots in terms of the sines of angles dependent on the revolution of Venus, the Earth, Jupiter, and Saturn, round the Sun. The coefficients of the sines in the terms of this formula are in each case the mass of the planet divided by the square of its mean distance. According to this formula, the spots are determined as to number and frequency by the position of these four planets: Jupiter preponderates and determines the length and height of the undulation of the curve; the action of Saturn shows itself by small variations; Venus and the Earth transform the line of simple undulation into a zigzag vibration." The Rev. R. Walker adds that the "form appears to be *entirely empirical*," which I believe to be the case.

The excess of 7 or 8 months in the sidereal period of Jupiter over the supposed solar-spot period of 11.1 years, becomes also a considerable discrepancy in the course of a moderate term of years; and his most favourable conjunctions with other planets do not seem very constantly to agree with the periods given for solar-spot maximum during the present century. The periods, too, for Jupiter in perihelion, supposing that position naturally to favour an increase of magnetic action or attraction in the sun,

* See his discourse "On the Physical Constitution of the Sun," read before the late Meeting of the British Association at Oxford.

do not agree—in fact rather the reverse—with the nearest periods of spot-*maximum*; *e. g.* Jupiter was in perihelion in

July 1809	1816	} Years of solar-spot maxima.
May 1821	1828	
April 1833	1837	
Feb. 1845	1848	
Dec. 1856	1860	

There are then certainly considerable difficulties in the way of accounting for the spot-period of 11.1 years, if we suppose that Jupiter's sidereal period of 11.8 years necessarily regulates it.

Being, however, strongly convinced in my own mind that planetary influences would necessarily have some effect, one way if not in another, upon the sun's physical constitution, and probably influence the phenomenon of periodicity in spot-*maximum* and *minimum*, I requested a person of my acquaintance, naturally endowed with a mathematical and astronomical turn of mind, to see if he could not find some synodical period answering to the secular term of 11.1 years. I advised him, in the first place, to investigate the period of Jupiter, and his conjunctions with Saturn, the Earth, and Venus; in the second place, to take likewise into especial consideration their positions in their own orbits as regards least distance from the sun, and the times when they were in line with the sun's equator. He says in reply, in one of his letters to me, "I have failed in finding out some configuration of the planets to account for the spot-cycle of 11.1 years; I can find nothing that comes at all near it, but the return of Jupiter to the same point in his orbit once in about 12 years; and this epoch is without any certainty of either conjunction or opposition with any of the other planets. Jupiter passes Saturn at periods of about 20 years, and Uranus at periods of about $14\frac{3}{4}$ years; and he is passed by the smaller planets at much shorter periods than 11 years. And it does not appear that this particular point in the orbit of Jupiter, where he is situated at the time of spot-*maximum*, is either the perihelion, aphelion, or either of his nodes. The perihelion of Jupiter is in Aries, and he was last in that point (the nearest point to the sun) about December 1856; and as the line of apsides moves very slowly, he will return to the same point again in 1869, and so on every 12 years for many returns."

On the 9th of September last I received from him the following letter, which I give *verbatim*, as being, at least, curious and interesting:—

"Dear Sir,—In searching for the solar-spots cycle, in some arrangement of the planets, I hit upon a rather remarkable conjunction, which is as follows:—

"Supposing the sun to make one revolution on his axis in 25

days 8 hours, say 25·3 days; then in the time that the sun makes 160 revolutions on his axis, Mercury will make almost exactly 46 revolutions in his orbit, and Venus 18; and this singular conjunction will recur at periods of about 11 years and 31 days, or about 11·08 years, which does not differ materially from the solar-spot cycle of 11·1 years. The statement is as follows:—

	Revolutions.	Time of Revolution.	Days.
Sun	160 (on axis)	× 25·30	=4048
Mercury . . .	46 (in orbit)	× 87·97	=4047
Venus	18 „	× 224·70	=4045
Earth	11·08 „	× 365·25	=4047

“It appears, then, that in periods of about 11·08 years a conjunction of Mercury and Venus takes place with the same side of the sun presented to them. This phænomenon may occur at several places in the ecliptic, one of which happens to be at the perihelion of the orbit of Mercury, and also within a few days of his passing the ascending node. Now the orbit of Mercury is considerably more eccentric than that of any of the other planets—as much as one-fifth of his semi-major axis; there must therefore be a considerable increase in his attractive energy at that particular point of his orbit; this, added to the fact that he is very nearly in a line with the sun’s equator (being nearly in the node), combined also with the conjunction of Venus, would tend to increase the amount of attraction acting on the sun’s surface at that time. This singular conjunction will sometimes be strengthened by that of some of the other planets; but this will only be a casual occurrence. For instance, at the last conjunction, which happened in March last, Jupiter was less than 30° from that point of the ecliptic; he should therefore have some influence. At the next return of the period Jupiter will be almost, if not quite, in exact conjunction with Mercury and Venus; this will take place in April 1871. The following statement shows the times when this phænomenon has last occurred:—

November	1815.
December	1826.
January	1838.
February	1849.
March	1860.

“These years, I believe, do not exactly agree with the years of maximum spots, but yet so nearly as to challenge further attention to the coincidence, and induce one to think that the phænomenon will have something to do with them; if so, I should be inclined to think the occurrence of spots, in a lesser degree, should be more frequent, viz. at intervals of 88 days, which is the time in which Mercury goes from his perihelion to his peri-

helion again ; now, he passed his perihelion in June last, and there was an extraordinary number of spots at that time ; since then the sun has been comparatively free from spots. Mercury is in perihelion now ; and I was observing the sun on Friday, and the spots had increased very much both in number and size.

"I remain, dear Sir,

"Quarry Bank, near Wilmslow,
Sept. 8th, 1860."

"Yours truly,

"J. HENSHALL."

Should future investigation prove that this peculiar conjunction of Mercury and Venus is something more than a mere coincidence, taken in connexion with the interval of 11.1 years between the solar-spot *maxima*, it will not fail, I think, to create surprise that two such small planets should produce apparently so strong an influence upon the sun, as that required to regulate the periodicity of his spot-energy ; it would have been more natural to suppose that the intensity of the planetary disturbance or effect would at least have been to some extent proportional to the masses, and inversely as the squares of the distance. One can only suppose that Mercury (and probably Venus similarly, though to a minor extent), owing to his extreme density, possesses a metallic constitution highly favourable to electro-magnetic action ; he may indeed intrinsically be a more powerful magnet than all the rest of the planets put together. His proximity to the sun, great eccentricity of orbit, as well as considerable inclination of orbit to the ecliptic, may help to favour his magnetic energy upon the sun, direct or reflex ; while generally it may perhaps be found that whatever magnetic action may exist between a planet and the sun, it will be most powerful when the planet is in one of its nodes, in a line with the sun's equator. This, at least, we should be inclined to imagine would be the case, should the solar magnetic poles coincide with his poles of axial rotation, and consequently in a direction perpendicular to the supposed lines of planetary magnetic force. It is worthy of remark that Jupiter passed his ascending node about the 1st of September last year, about the time of the great magnetic auroral disturbance, at a time likewise when the spots and markings on his own surface were remarkably numerous and distinct, and also near the time of solar-spot maximum.

Manchester,
Sept. 18, 1860.

Yours respectfully,
R. P. GREG.

Note.—Since writing the above, I have received a short note from Mr. Carrington, to whom I wrote respecting the peculiar conjunction of Mercury and Venus, mentioned by Mr. Henshall ; and I am inclined to agree with him, that it would be natural to expect nearly as decided solar effects from the ordinary and more frequent conjunctions of Venus and Mercury. There is likewise

a difficulty in knowing the precise time for a revolution of the sun on his axis, the period varying (I presume as measured by the spots on his surface) as much as two days in certain regions or latitudes, in excess of the equatorial. Still the influence and position of the planet Mercury may have considerable magnetic effects.

It must also be borne in mind, while attempting to connect planetary influences through mass-attraction, with the secular returns of spot-*maximum* and *minimum*, that those periods are not, as far as they have hitherto been examined, precisely regular or fixed; the average, taken from before 1800 to 1860, gives, according to Wolf, a period of 11.1 years; but from the years 1826 to 1850, the best observations made by Schwabe show that the period was then a decennial one, with *maxima* in 1828, 1837, and 1848, and *minima* in 1833 and 1843; and Sabine states "that during the same period the solar magnetic variations show likewise a decennial period strictly coincident with the solar spots."

XXXIV. On the Elastic Force of Vapours. By M. REGNAULT.

[The important researches of M. Regnault on the Elastic Force of Vapours under various conditions, abstracts of which have been given in the Numbers of this Magazine for October 1854 and January 1855, have just appeared in full in the twenty-sixth volume of the *Mémoires de l'Académie*. In calling attention to their publication, in the *Comptes Rendus* for June 11, 1860, the author at the same time communicates an account of some further developments which he has given to a part of this research—that which treats of the tension of saturated vapours *in vacuo*, a translation of which we proceed to lay before our readers.—Eps.]

The various apparatus which I have used in these researches are described in the original memoir. I will merely remark that they refer to two different methods.

The first, which I call the *static* method, consists in determining the pressure which is equal to the elastic force of a vapour *at rest* disengaged from a liquid in excess. In the second method, which is called the *dynamic method*, the vapour is always in motion, and a determination is made of the temperature of the vapour *continually* emitted by the liquid boiling under different pressures.

Both methods give the same results,

1. When the liquid is quite homogeneous. This is not the case when it is impure; the presence of the smallest quantity of a volatile foreign body is immediately seen in the non-superposition of the two graphic curves which belong to one or the other of these methods.

2. Where the liquid does not possess a great molecular cohesion; for otherwise the liquid boils in an intermittent manner with violent jumpings, and the determinations by the dynamic method become very uncertain.

Both methods have been successfully applied to most of the volatile liquids taken for experiment, and I have been able to determine their tensions from the lowest temperatures to those which correspond to pressures of 12 to 15 atmospheres. Most liquefiable gases give liquids having a great molecular cohesion, and which boil with great difficulty in spite of their extreme mobility. Their tensions can only be determined with certainty by the statical method. In order to apply the dynamic method—that of ebullition—the thermometer can only be placed in the vapour of a boiling liquid when its boiling-point is above that of the surrounding atmosphere, for otherwise the vapour might become superheated, and the indications of the thermometer would be faulty. If the thermometer dips in the boiling liquid, it exhibits variations of temperature during ebullition, although the pressure remains the same. The indications of the thermometer often change according to the manner in which the heat is applied. The ebullition is not continuous; violent jumpings take place accompanied by a sharp sound like that emitted by inverting the water-hammer. These effects greatly vary with the pressure. In certain liquids they take place at pressures below that of the atmosphere. In others they only take place under high pressure.

In this limited space I must omit any mention of the observations I have made on each substance, of the graphic method of constructing the results, and of the interpolation formula by which I have endeavoured to represent my researches. I will only say that, of all the methods of interpolation tried, the exponential formula proposed by Prony, and applied by Biot to aqueous vapour under the form

$$\log F = a + 6b\alpha^t + c6^t,$$

applies best to all the substances I have tried. This formula has the advantage of containing five constants, for the determination of which, five points of the curve may be chosen having equidistant abscissæ, so that the curve represented by the formula can deviate in the intermediate parts very little from the graphic curve. For a great number of the substances which I have studied, by a convenient arrangement of the fixed points which serve to calculate the constants, and without deviating too much from the observed data, a formula with two exponentials may be calculated,

$$\log F = a + b\alpha^t + c6^t,$$

in which the term $c6^t$ only introduces values which are less than

the probable errors of observation, so that it can be reduced to the much simpler formula

$$\log F = a + b\alpha^t.$$

This consideration, and the great resemblance which the graphic curves belonging to different substances bear to each other when

$\log \frac{F}{760}$ is taken for ordinates, lead me to think that the law of

variation of the tensions of vapours with the temperatures would appear under a very simple form by taking as an independent variable, not the temperature such as we define it in a completely arbitrary manner, but another element which would be in a direct relation with the constitution of each body, and whose origin would be fixed for each of them.

The Tables of the elastic forces of the vapours of alcohol, ether, chloroform, bisulphide of carbon, and essence of turpentine have already appeared in this Journal. In the new Table these tensions are determined for every 5 degrees; and there are further determinations of the tensions of benzole, quadri-chloride of carbon, C^2Cl^8 , of chloride, bromide, and iodide of ethyle, of methylic alcohol, essence of lemon, acetone, and oxalic ether. In the following Tables we shall give the tensions of mercury vapour, and also of some liquefied gases.

Mercury.		Mercury.	
T.	F.	T.	F.
	mm		mm
0.0	0.0200	270	123.01
10	0.0268	280	155.17
20	0.0372	290	194.46
30	0.0530	300	242.15
40	0.0767	310	299.69
50	0.1120	320	368.73
60	0.1643	330	450.91
70	0.2410	340	548.35
80	0.3528	350	663.18
90	0.5142	360	797.74
100	0.7455	370	954.65
110	1.0735	380	1136.65
120	1.5341	390	1346.71
130	2.1752	400	1587.69
140	3.0592	410	1863.73
150	4.2664	420	2177.53
160	5.9002	430	2533.01
170	8.0912	440	2933.99
180	11.00	450	3384.35
190	14.84	460	3888.14
200	19.90	470	4449.45
210	26.35	480	5072.43
220	34.70	490	5761.32
230	45.35	500	6520.25
240	58.82	510	7353.44
250	75.75	520	8264.69
260	96.73		

The temperatures refer to the air-thermometer. The ebullition of mercury is regular enough at pressures below that of the atmosphere. Under the atmospheric pressure, jumpings commence, and they increase in proportion as the pressure increases: under a pressure of ten atmospheres the shocks are so violent that they produce a noise like that of a hammer striking an anvil. Every moment the apparatus may be expected to burst into pieces.

Very volatile Liquids.				Liquefied Gases.	
Sulphurous acid.		Ammonia.		Sulphuretted hydrogen.	
T.	F.	T.	F.	T.	F.
	mm		mm		mm
-30		-78°2	157·95	-78°2	441·52
-25	373·79	-40	528·61	-30	2808·57
-20	479·46	-35	684·19	-25	3508·02
-15	607·90	-30	876·58	-20	4273·01
-10	762·49	-25	1112·12	-15	5090·18
- 5	946·90	-20	1397·74	-10	5945·00
0	1165·06	-15	1740·91	- 5	6822·74
+ 5	1421·14	-10	2149·52	0	7709·27
+10	1719·55	- 5	2632·25		
+15	2064·90	0	3162·87		
20	2462·05	+ 5	3854·47		
25	2915·97	10	4612·19		
30	3431·80	15	5479·86		
35	4014·78	20	6467·00		
40	4670·23	25	7581·16		
45	5403·52	30	8832·20		
50	6220·01	35	10144·00		
55	7125·02	40	11776·42		
60	8123·80				
65	9221·40				

The condensation of the gases was effected in the same apparatus which served to determine the tensions, and which was so arranged as to be quite freed from the last traces of air or any other gas which it might contain. The liquefaction of sulphurous acid is readily effected at the ordinary atmospheric pressure, when the apparatus is placed in a cooling mixture. For ammonia and sulphuretted hydrogen the apparatus is placed in a mixture of ice and crystallized chloride of calcium, and then the gas compressed with a hand force-pump. Care must be taken that the ordinary grease of the pumps is replaced by fixed oils which are not saponifiable. By a pressure of 2 to 3 atmospheres the desired quantity of liquid ammonia is obtained; but for sulphuretted hydrogen the pressure must be raised to 7 or 8 atmospheres.

I have had occasion to liquefy these gases on a large scale for researches (of which I shall presently lay the results before the

Academy) on the determination of the latent heats of evaporation, under different pressures, of very volatile liquids, and on the quantities of heat which these gases absorb under pressure. I shall briefly describe the method adopted.

I prepare carbonic acid by a regulated and continuous addition of dilute hydrochloric acid to broken marble placed in a large flask. The solution, freed from acid and charged with chloride of calcium, flows out as fast as it is produced, and the gas passes into a gas-holder of a cubic metre capacity. A force-pump, with several barrels, and worked by my steam-engine, draws the gas in the gas-holder, having made it previously traverse several drying materials. It forces the gas into a first receiver of 3 or 4 litres capacity, which simply serves as regulator. The gas then passes freely into the apparatus in which it is to be condensed, and which is placed in a mixture of ice and crystallized chloride of calcium. The non-condensed gas passes into a second closed receiver of 5 litres, placed at the end of the apparatus. This serves to receive the air and the non-liquefiable foreign gases, which can be allowed to escape from time to time by opening the stopcock.

Protoxide of nitrogen and bisulphuretted hydrogen can be liquefied in large quantities by the same arrangement. But for gases which readily alter in contact with fatty matters, I employ a special force-pump, in which the gas is only in contact with mercury. This pump consists of two equal barrels of cast iron, joined so as to be U-shaped. The first barrel contains a solid piston, which in its motion simply acts on a quantity of mercury which exactly fills one of the barrels. The system of two valves, exhausting and forcing, is arranged in the second barrel. It is evident that by this means the gas never comes in contact either with the piston or the greased sides.

I have been especially interested in liquid ammonia, in consequence of its great calorific capacity, of its great latent heat of evaporation, and of the ease with which it is obtained and afterwards collected in the gaseous state. I propose to use it principally in obtaining very constant low temperatures by causing it to boil under different pressures. I prepare ammoniacal gas by causing a stream of strong liquid ammonia to fall into a copper vessel contained in a small boiler in which water is kept boiling by means of a gas-lamp. The vessel is consequently always surrounded by the vapour of boiling water; the ammonia flows in a spiral along the sides; and the liquid, which is almost freed from ammonia, runs out by a tube at the bottom, which plunges to a depth of several decimetres in cold water. The ammoniacal gas exhausted by the pump passes through several copper receivers full of fragments of soda-lime;

the pump regulates the production of gas, and drives it into a receiver placed in a mixture of crystallized chloride of calcium and ice. In this way many litres of liquid ammonia may be prepared in a few hours.

In order to expose an apparatus to a stationary low temperature, it is hermetically adjusted in the receiver; this receiver is placed in the freezing mixture, and ammoniacal gas liquefied in it. When it is sufficiently full of the liquid gas, the freezing mixture is removed, and the receiver connected with one of my large reservoirs of air, by which the pressure can be kept perfectly constant, either greater or less than that of the atmosphere. The ammonia distils in this manner at as low pressures as can be desired; and it is easy to keep them perfectly constant provided the ammoniacal gas is prevented from reaching the air-reservoir. For this purpose a cylindrical vessel containing pieces of ice is placed before the reservoir; the ice in liquefying almost entirely redissolves the ammonia, and the little which escapes is caught in a second vessel full of pumice soaked in acid.

By this arrangement I hoped to obtain low temperatures perfectly stationary, but have been disappointed, for the reasons stated above (page 276). A certain regularity can only be obtained when a continual stream of small air-bubbles is passed into the liquid ammonia through a rose, which continually agitates the liquid, and destroys its viscosity. An air-thermometer ought to be connected with the apparatus. By means of a regulating screw, the supply of air-bubbles can be regulated so as to keep the thermometer stationary.

XXXV. *A Theory of the Force of Electricity.*

By Professor CHALLIS.*

AFTER an interruption occasioned by close occupation with astronomical work, I propose now to resume the explanation of my theory of the physical forces. The principal hypothesis of the theory is, that the physical forces are all consequences of the motions and pressures of a uniform and highly elastic medium pervading space. The variations of the pressure of the medium are supposed to be proportional to variations of its density; and this supposition forms the basis of a mathematical investigation of the relations between the motions and the pressures. Further, it is assumed that the medium acts immediately by pressure on the ultimate atoms of bodies, which are all supposed to be spheres of invariable magnitudes, and of the same intrinsic inertia. According to these hypotheses, the different phenomena and properties of bodies

* Communicated by the Author.

depend only on the *magnitudes* of their atoms, the *proportions* in which they are composed of atoms of *different* magnitudes, and on the *arrangements* of the atoms. But the explanation of the sensible properties of matter must be preceded by a mathematical investigation of the laws of the dynamic action of the assumed ætherial medium. Already I have entered to some extent on such investigations relatively to light, heat, the force of gravity, and the forces of molecular aggregation. I proceed now to treat of the force of electricity on the same principles. First, however, it should be stated, with respect as well to this investigation as to those which preceded it, that I do not profess to give complete and final explanations of the laws of the forces under consideration. As the proposed general theory either embraces *all* the physical forces, or fails altogether, my object at present is to adduce *primâ facie* evidence that it is as comprehensive as it is required to be.

1. According to the "Theory of Molecular Forces" contained in the Number of the Philosophical Magazine for last February, the atoms of any substance are kept in positions of equilibrium by attractions and repulsions resulting from the dynamical action of vibrations of the æther, which have their origin at the atoms. Each atom is the centre of vibrations propagated from it equally in all directions, and that part of the velocity of the vibrations which, as shown by the mathematical formulæ, is unaccompanied by change of density, gives rise to a *repulsive* action on the surrounding atoms, varying inversely as the fourth power of the distance. This action is the *calorific* repulsion which keeps the individual atoms asunder. The remaining part of the velocity, which is accompanied by a proportional condensation, acts *repulsively* also, according to the law of the inverse square, but with a force which at the surface of the atom is incomparably less than the other repulsion. When, however, the effect of all the repulsions of the second kind from an *aggregation* of atoms, constituting a spherical *molecule*, is considered, the resultant may be of sensible magnitude at sensible distances from the centre of the molecule. To this *molecular* action the repulsion of *aëriiform* bodies is attributed in the theory referred to. It appeared also from the theoretical investigation, that the separate waves from the atoms of a spherical molecule of a higher order of magnitude, consisting of a number of atoms very much larger than those constituting the molecule of the first order, might merge at a certain distance from its centre into continuous waves of another order of breadth and condensation, the dynamic action of which might be such as to account for the molecular *attraction* of aggregation in *fluids* and *solids*.

For the sake of distinction, the first of the above-mentioned
Phil. Mag. S. 4. Vol. 20. No. 133. Oct. 1860. U

forces, that which is described as emanating from individual atoms, will be called *atomic repulsion*; the second, or that emanating from the smaller aggregation of atoms, will be called *molecular repulsion*; and that emanating from the larger aggregation, *molecular attraction*. The last two are the forces which will chiefly come under consideration in the following theory of electricity.

2. In the ordinary or neutral state of any substance, the sphere of activity of each of these forces appears to be extremely small. Hence if a plane be conceived to pass through the position of an atom situated in the interior of a uniform medium, not very close to its boundary, the forces of each kind, emanating from the atoms on opposite sides of the plane, just counteract each other, and the atom is thus maintained in a position of stable equilibrium. But an atom situated at the boundary of the substance is held in equilibrium by the equal and opposite actions of the attractive and repulsive forces. As the sphere of activity of the attraction was shown in the theory to be much greater than that of repulsion, and to vary much more slowly with the distance, the equality of the attraction and repulsion on a superficial atom may be supposed to be the consequence of a rapid degradation of the density of the substance towards the bounding surface. The effect of this would be, to diminish the resulting repulsion on a superficial atom from within to without, so as to reduce it to an equality with the resulting attraction from without to within, which is less affected by the gradation of density at the surface, on account of its larger sphere of activity. It is possible that the diminution of density may be such that at the boundary the *atomic* repulsion becomes of very small magnitude, as is the case, according to the theory, in the interior of aëriiform bodies, by reason of the comparatively large intervals between their atoms. Supposing, however, the body to be surrounded by air, the atomic repulsion must be such as would suffice to counteract the pressure of the air. The *molecular* repulsion would then be counteracted by the molecular attraction, excepting so far as a slight excess must be attributed to the latter very close to the surface, in order to account for the phenomena of capillary attraction. As these forces both vary with the distance from their origins, according to the law of the inverse square, their resultants would neutralize each other at all sensible distances from the surface of the substance from whose atoms they emanate. In this manner the theory accounts for the fact that the molecular forces of bodies have in general no perceptible effect at sensible distances from the boundaries.

3. Now this equality between the superficial molecular repul-

sion and the superficial molecular attraction would be destroyed by any extraneous action, such as the friction of one substance against another, by which the relative positions of the atoms would be changed, and the normal state of the superficial degradation of density be disturbed. Such disturbance would produce the *electric* state. If the result of the disturbance is to cause the molecular attraction to be in excess of the molecular repulsion, the electricity is of a certain kind, different from that in which the molecular repulsion is in excess. The theory thus accounts for the existence of *two kinds of electricity*, and for the fact that electricity is *confined to the surfaces of bodies*.

4. It seems evident that, according as the superficial molecular attraction is *greater* or *less* than the superficial molecular repulsion, the superficial density is *greater* or *less* than that in the neutral state. It does not, however, readily appear to which of the two kinds of electricity the denser, and to which the rarer, condition of the superficial strata applies. But since experiment indicates that when two substances are electrified by friction, that which is the more yielding, or more susceptible of molecular displacement, is negatively electrified, it may perhaps be inferred that in the case of *negative* electricity the superficial density is *greater* than in the neutral state, and the molecular attraction in excess; and in that of *positive* electricity, the superficial density is *less* than in the neutral state, and the molecular repulsion in excess.

5. Upon the cessation of the friction it might be expected that the superficial atoms would quickly return to the neutral positions, and the normal gradation of density be resumed. What, then, is the reason that the electric state of a substance is found by experiment to continue with little abatement long after the exciting cause has ceased to act? The answer to this question involves the consideration of the part which the *air* performs in electrical phenomena, which, as experience shows, is so essential that no theory of electricity can be complete which does not give an account of it. The explanation which the present theory offers is as follows. When the law of density of the superficial strata has been altered by the disturbing action of friction, and the equality of the molecular attraction and repulsion is destroyed, so that, for instance, the former exceeds the latter, immediately the surrounding air is attracted towards the electrified body, and condensed on its surface, and at the same instant an equilibrium is established at the surface between the repulsion of the condensed air and the *atomic* repulsion of the body. Considering the rapid change of the atomic repulsion with distance, and the small sphere of its activity, this equalization of the two repulsions may be the result of a change of the

superficial density extending to a distance from the surface incomparably smaller than the distance to which the original disturbance affected the atoms. Consequently the excess of the molecular attraction above the *molecular* repulsion, to which that disturbance gave rise, will continue so long as the condensation of the air is undisturbed, and thus the body will remain in a state of *negative* electricity.

6. Similarly, if the friction disturbs the atoms in such a manner that the superficial molecular repulsion is in excess, the surrounding air is repelled from the electrified body, and the parts contiguous to the surface are rarefied. At the same time, the *atomic* repulsion of the body being equalized, by an extremely small change of the superficial density, to the repulsion of this rarefied air, the excess of the *molecular* repulsion above the molecular attraction remains, and the body continues in a state of *positive* electricity.

7. It is evident that the air can operate in this manner only to a limited extent, depending on its density, and that the amount of the permanent electricity, of which any substance is capable, is limited by this circumstance. The electricity will be permanent in proportion as the air adheres to the body without being replaced by other air. For this reason dry air is more favourable to the permanence of electricity than air containing vapour of water, which being only connected with the air mechanically, and not a constituent part of it, is liable to relative displacement by extraneous causes.

8. As the excess of molecular attraction, or of molecular repulsion, acts through considerable spaces, the resulting condensation or rarefaction of the air at any point of the surface of the electrified body; depends on the curvature of the surface at that point, being less as the curvature is greater. This explains *the escape of electricity at sharp points*, where on account of the great curvature of the surface, the sum of the molecular attractions or repulsions is too feeble to produce a degree of condensation or rarefaction sufficient to maintain the electric state.

9. When two substances are rubbed against each other, the external force applied to cause the friction, by putting the contiguous superficial atoms of both into states of disturbance, gives rise to mutual molecular actions, which in fact determine the new positions that the atoms assume. Now, whatever be the resultant of these actions on a given atom, it is evident that those atoms of one of the bodies which are most nearly contiguous to atoms of the other, must be acted upon by resulting forces very nearly of the same magnitude and in the same direction as the forces which act upon the latter. Hence, if for one of the substances the resulting force be from within its boundary to without, for the

other it must be from without to within; and if in one case the superficial density is made less than it is in the neutral state, in the other it is made greater. Thus the fact that *two bodies take by friction opposite electricities*, is in accordance with this theory. The circumstances which determine one to take the positive electricity and the other the negative, depend most probably on differences of atomic constitution and arrangement. But this is a question distinct from the present inquiry, which is concerned only with the kind of agency by which electric phenomena are produced.

10. A *good conductor* of electricity may, in the language of the theory, be defined to be a substance which has the property of transmitting along its surface the effect of a disturbance made at any point of the same, so that the law of superficial gradation of density which the disturbance induces obtains equally over the whole of the surface. In *bad conductors* and *non-conductors* the change of the state of superficial density is transmitted slowly, or not at all, and the gradation of density may at the same instant be different at different points of the surface. What the constitutional difference may be between a conductor and a non-conductor, it does not belong to the present theory to inquire.

11. The tenacity with which the electric state is maintained, depends not only on the condition of the air, but also on the *form* of the surface of the electrified body. We have already seen that the condensation or rarefaction of the air contiguous to the body, which in fact keeps it in the electric state, is greatest where the curvature is least, and that it is very small at sharp points. This explains what has been called the *accumulation* of electricity at points of great curvature, which, according to this theory, is only a less degree of retention of the electricity at such points, and a greater tendency to be affected by the approach of a disturbing cause.

12. The uniform distribution of electricity over the surface of a good conductor may be modified by circumstances, the consideration of which leads to the theory of *electricity by induction*. Conceive a good conductor, whose form, for the sake of simplicity, we will suppose to be spherical, to be brought within the influence of another sphere in a state of *negative* electricity. As, according to what has been said, the dynamic action of the latter attaches to itself the surrounding air, and the extent of its influence, as is known by experiment, may be very considerable, it may be supposed to act attractively also on the other sphere, or at least on the superficial atoms that are turned towards it. I do not gather from experiments, that electric action can pass freely through the substances of bodies as the force of gravity

does, so that the opposite side of the sphere in the present instance could be *immediately* acted upon by the first sphere. The fact rather seems to be that a *partial* disturbance of the normal atomic condition of a body, at whatever part it takes place, is transmitted through the intervention of its proper molecular forces to all other parts, so that the relative positions of the atoms both in the interior and contiguous to the boundaries are in some degree changed. This remark may be illustrated as follows. Suppose a cylindrical rod to be held by the hand near its middle point, and to be moved in the direction of its length. The force which causes the motion is applied at certain parts only; but if we admit that the rod is composed of discrete atoms, the disturbance immediately impressed gives rise to molecular action which extends throughout the substance of the rod, causing each atom to move in the same direction and through the same space as the rod itself. If we consider an atom at the preceding extremity of the rod, it is evident that its motion must be due to an excess of molecular repulsion from within to without above molecular attraction in the contrary direction, so that, according to the theory, this end would be in a state corresponding to positive electricity. At the other extremity the motion of an atom must be caused by an excess of molecular attraction from without to within above molecular repulsion in the contrary direction, and the superficial condition would correspond to that of negative electricity. At the same time there must be a gradual increase of density from the preceding to the following end, to account for the movements of the interior atoms by molecular action, there being no such action in the interior if the density be uniform.

13. Now it may be presumed that the electrified sphere, in the case before us, by acting partially on the other sphere, that is, by attracting only the superficial parts which are turned towards it, produces effects on the whole sphere analogous to those above described. By attracting towards itself the atoms on which it immediately acts, it conspires with the repulsion from within to without of the other sphere, and thus produces an apparent excess of repulsion, and consequently a *positive* state of electricity. Thus the electricity *directly induced is contrary to that of the electrified sphere*. At the same time, since all the atoms of the sphere submitted to the electric influence must tend to move in the direction of the partial impulse, and since those of its atoms which are not immediately acted upon can only move by its proper molecular action, it follows, just as in the case given above for illustration, that there must be a gradual increase of density from the nearer to the further side of the sphere to produce internal molecular action in the direc-

tion of the immediate impulse; and at the boundary furthest from the electrified sphere, there must, for the same reason, be an excess of molecular attraction from without to within above the opposite molecular repulsion. Thus the further side of the sphere is electrified *negatively*. The opposite electricities would plainly have been induced if the first sphere had been electrified *positively*. These inferences are in accordance with well-known results of experiment.

14. The induced electricity is maintained by the presence of the electrified sphere, which supplies the pulling or pushing force by which the electric condition of the other sphere is generated. On the removal of the former, the electric state of the latter would immediately vanish, for the same reason that, in the case of the rod, as soon as the hand ceases moving it, the molecular action also ceases.

15. It has been supposed in the above reasoning that the body on which the electricity is induced, is insulated by a non-conductor, and is of limited extent. If it were connected by a conductor with the earth, it must be considered, as far as regards its electric state, to be a body of unlimited extent, and in that case the argument which accounted for induced electricity on the more remote parts of the same kind as that of the electrified body would no longer apply. This electricity would be removed, as it were, to an infinite distance, and the only induced electricity would be that of the opposite kind.

16. It is found by experiment that if a spherical shell of metal, perforated at one part, be electrified, little or no electricity is found on the interior surface. This fact is accounted for by the neutralization of the electricities on opposite parts of that surface by *induction*. The same explanation applies in the case of an ingenious experiment made by Faraday, in which an electrified hollow cone, having an acute vertical angle, was turned inside out by pulling a thread. The electricity was always on the surface which was exterior, that on the interior surface being neutralized by induction.

17. An *electric discharge* takes place when on the approach of two bodies oppositely electrified towards each other, the *condensation* of the air due to the *negative* electricity of one is neutralized in the space between them by the *rarefaction* of the air due to the *positive* electricity of the other. The *spark* is probably a consequence of the disturbance of the æther caused by the sudden return of the superficial atoms of both bodies, at the instant of the discharge, to their normal positions.

18. It might at first sight appear that the theory which accounted for induced electricity would also explain the attractions and repulsions by which bodies in a state of induced elec-

tricity are found to act upon each other so as to produce motions of translation. But these motions cannot be due to the attractions and repulsions which have hitherto been under consideration; for if so, an electrified body, according as its electricity is *negative* or *positive*, would *attract* or *repel* another on which it induces electricity. Whereas the fact is, attraction takes place in both cases, and it is only after the bodies have been in contact, and their electricities have become of the same kind, that they mutually repel. Also the attractions and repulsions hitherto considered act directly upon only an extremely small portion of the whole mass, and for this reason must have a feeble translating power. If they are at all operative in producing motions of translation, such motions must be veiled by the action of more powerful forces of a different kind. It is clear that forces by which two bodies at one instant attract, and at the next repel each other, cannot be intrinsic or resident in the bodies themselves, but must be attributed to the dynamic action under different conditions of the agent to which the force of electricity is ascribed. Now we have just seen that the dynamic action which the ætherial medium—the agent assumed in this theory—exerts by means of *waves*, fails to give the requisite explanation, and we are thus restricted to the supposition that it acts by *currents*. This, I think, will be found to be the true explanation. That there exist *steady* ætherial currents, is indicated by magnetic phenomena, as I hope at some time to show. Assuming the existence of such currents, in *whatever* direction they traverse bodies, whose atoms are so arranged that the density increases with the distance from a fixed plane, *secondary* currents must be generated which flow from the rarer to the denser parts with a velocity depending on the magnitudes of the atoms, and the number in a given space. The reason for this assertion is derived from the hydrodynamical equation generally applicable to cases of steady motion, which is of this form,

$$\text{Pressure} = f(t) - \frac{V^2}{2},$$

showing that where the velocity is greater the pressure is less. But the velocity of a steady current is greater through the denser parts of a medium than through the rarer, because in the former the space the æther fills is less than in the latter, on account of the greater occupation of space by the atoms of the medium. Hence there is a constant accelerative force from the *rarer* towards the *denser* parts, generating a current in that direction. These secondary currents may be regarded as instances of steady motion to which the equation above is applicable.

19. Now it has been shown (in arts. 13 and 14) that in a

body electrified by induction there exists that gradation of density here supposed, and the condition required for generating a secondary current is consequently fulfilled. A body inductively electrified acts reciprocally on that by which its electricity was induced, and thus *both* are in a condition proper for generating secondary currents. If the electricities on the adjacent sides are of *opposite* kinds, the two currents are in the *same* direction, either towards the right hand or towards the left. And as the velocity of the streams diminishes rapidly with distance from the surfaces of the bodies by divergence and lateral spreading, there will be a position on the shortest distance between them at which the conspiring streams will produce a maximum of velocity, and consequently a minimum of pressure. Towards this point, therefore, there will be a tendency of both bodies to move by the action of the pressure of the medium on the individual atoms, and thus a reason is given by the theory for *the attraction towards each other of bodies oppositely electrified*.

20. If the electricities on the adjacent sides of the bodies be of the *same* kind, the currents will be in *opposite* directions, either *from* or *towards* the space between. In both cases there will be a minimum of velocity, and consequently a maximum of pressure somewhere in the shortest line joining the bodies, because there must be a point at which the difference of the opposite velocities is least. The maximum of condensation at this point is produced by the meeting of the currents when they flow towards it, and by the confluence of the adjacent fluid when they flow from it. Thus in both cases the pressure of the fluid will tend *from* the intermediate position, and will separate the bodies by pressure on their individual atoms. In this manner the theory accounts for the observed *mutual repulsion of two substances having the same electricities*.

21. As the efficacy of these currents must be in proportion to the molecular attractions and repulsions before treated of, to which they owe their origin, and as these forces vary inversely as the *square* of the distance, it follows that the effective force of the currents varies with the distance according to the same law. This result accords with experiment.

22. The above explanations give an account of motions of electrified bodies the same in kind as those observed. Whether the agency be sufficient in degree, cannot be readily determined. On this point it may be remarked, first, that the pressures do not act partially on the bodies, but, as in the case of gravity, on *every* atom, and for this reason they are the more likely to be of sufficient motive power; and again, the moving force of the pressure on a given atom, being proportional to $V \frac{dV}{ds}$, depends

on the magnitude of the increment dV of velocity corresponding to the increment ds of space, and the factor $\frac{dV}{ds}$ may be large, although V is small.

Cambridge Observatory,
September 20, 1860.

XXXVI. *Chemical Notices from Foreign Journals.*

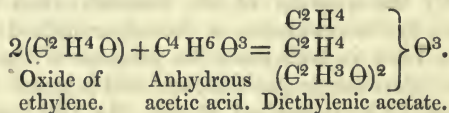
By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from p. 203.]

WURTZ* has continued his researches on oxide of ethylene, which have led to further interesting results.

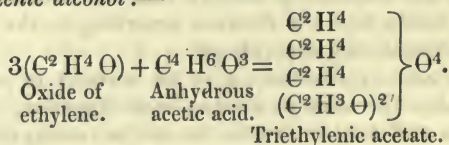
It combines directly with acids and neutralizes them. Oxide of ethylene also unites with acids, forming basic salts. When it is treated with anhydrous acetic acid, acetate of glycol is formed. When this body has been separated by fractional distillation, a considerable quantity of a liquid boiling above 200° C. remains. By working on large quantities, three products may be obtained from this liquid, which can be regarded as basic acetates of oxide of ethylene, and which really are acetates of polyethylenic alcohols†.

The first product boils at about 250° , and is the *acetate of diethylenic alcohol*, formed in accordance with the reaction:



When this compound is saponified by baryta, it is resolved into acetate of baryta and diethylenic alcohol, $\left(\begin{array}{l} \text{C}^2\text{H}^4 \\ \text{H}^2 \end{array} \right)^2 \text{O}^3$, or Lourenço's compound‡.

The second product boils towards 290° C, and is the *acetate of triethylenic alcohol*:—

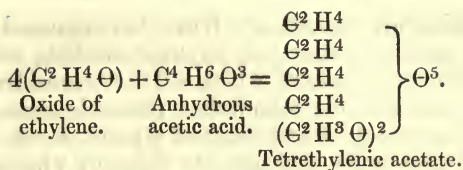


The third product, the *acetate of tetrethylenic alcohol*, boils at above 300° , and must be distilled *in vacuo*. It is a thick liquid, but perfectly colourless.

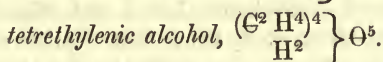
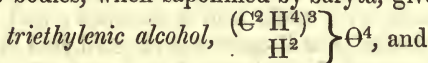
* *Comptes Rendus*, June 26, 1860.

† *Phil. Mag.* vol. xix. p. 124.

‡ *Ibid.* p. 122.

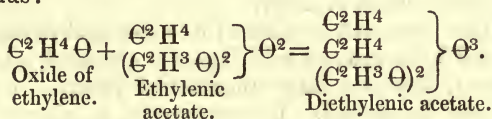


The last two bodies, when saponified by baryta, give respectively



Thus one, two, three, or four atoms of oxide of ethylene can unite with one atom of anhydrous acetic acid (which is equivalent to two atoms of hydrated acetic acid) to form in the first case a neutral acetate (diacetate of glycol); in the other cases acetates, more and more basic, which give rise to more and more complicated polyethylenic alcohols. The same acetates are formed by the action of ordinary acetic acid on oxide of ethylene, but in this case water is eliminated.

Oxide of ethylene unites directly with diacetate of glycol (ethylenic acetate) to form polyethylenic acetates—a reaction perfectly comparable to the transformation of acetate of lead into basic acetate when the neutral salt is digested with oxide of lead. Thus:



The basic properties of oxide of ethylene are well seen in its action on salts.

Oxide of ethylene mixes with a concentrated solution of chloride of magnesium; but after some time the mixture solidifies, magnesia is precipitated, and hydrochlorate of oxide of ethylene is formed. This latter body is decomposed by potash, with the formation of chloride of potassium and disengagement of oxide of ethylene. Consequently oxide of ethylene displaces magnesia from its compounds, but is in turn displaced by potash.

Oxide of ethylene precipitates hydrated sesquioxide of iron from a solution of perchloride. It precipitates alumina from a solution of alum, and subsulphate of copper from a solution of the sulphate.

Its basic properties are thus seen to be very distinct. Capable of neutralizing acids, of forming combinations with excess of base, and of displacing certain oxides, oxide of ethylene constitutes a true organic base, an alkaloid without nitrogen.

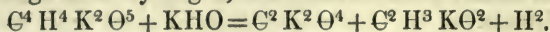
In continuing his researches*, Wurtz has examined the action of oxidizing agents on these polyethylenic alcohols, and has obtained results of great interest, which will throw considerable light on the constitution of the complicated vegetable acids.

By oxidizing glycol, Wurtz obtained glycolic, lactic, and oxalic acids by reactions quite analogous to those by which alcohol is transformed into acetic acid. By oxidizing diethylenic alcohol, an acid isomeric with malic acid is obtained; and by oxidizing triethylenic alcohol, a still more complicated acid is produced.

The oxidation of diethylenic alcohol was effected either by means of platinum-black, or by the action of nitric acid. In the latter case the reaction was very violent, and a tumultuous disengagement of nitrous fumes was observed. The acid liquid, evaporated to dryness, formed a mass of crystals. These crystals were dissolved in water, saturated with milk of lime, boiled, and separated by filtration from a deposit of oxalate of lime which had been formed. The liquor deposited, on cooling, a lime-salt, which crystallized in long brilliant needles. Dried at 170° , these crystals had the composition of a neutral and dry malate of lime. The formula of the crystals is $\text{C}^4 \text{H}^4 \text{Ca}^2 \text{O}^5 + 6 \text{Aq}$; and they do not completely lose this water below 160° . They are almost insoluble in cold, and are difficultly soluble in boiling water. The boiling saturated solution gives with nitrate of silver a granular precipitate, whose composition is $\text{C}^4 \text{H}^4 \text{Ag}^2 \text{O}^5$, which is the formula of malate of silver.

When this silver salt is diffused in water and decomposed by sulphuretted hydrogen, an acid liquor is obtained, which, when concentrated, deposits large rhomboidal prisms, very soluble in water and in alcohol. Its composition is expressed by the formula $\text{C}^4 \text{H}^6 \text{O}^5 + \text{H}^2 \text{O}$. The new acid effloresces slowly in the air, and loses an atom of water of crystallization, which it parts with much more rapidly *in vacuo*, or at 100° . The dry acid melts at 148° , and solidifies to a crystalline mass. At about 250° to 270° it decomposes, with disengagement of a combustible gas; the residue is a strongly acid liquor, which after some time solidifies to a crystalline mass, and which is a true pyrogenic acid.

The new acid has the composition of malic acid, but differs from it in form, in the amount of its water of crystallization, in its efflorescing, and in comportment when heated. In one respect it strongly resembles malic acid; fused with hydrate of potash it gives off hydrogen, and forms acetic and oxalic acids:—



Potash salt.

Oxalate of
potash.

Acetate of
potash.

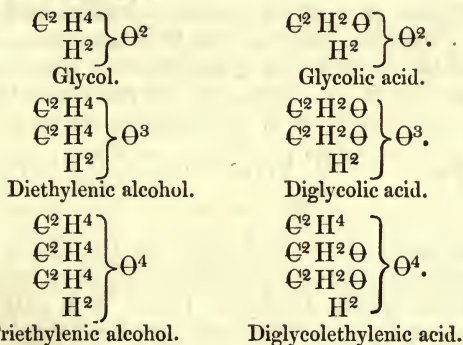
When a solution of the acid is half-neutralized with potash,

* *Comptes Rendus*, July 30, 1860.

an acid potash salt is obtained which closely resembles cream of tartar. The salt is anhydrous, and contains $\text{C}^4 \text{H}^5 \text{K} \text{O}^5$.

The oxidation of triethylenic alcohol is accompanied by the same phenomena as that of diethylenic alcohol. By suitable treatment two lime-salts are obtained: one, slightly soluble in cold water, is identical with that just described; the other is more soluble in water, and forms silky tufts like asbestos. The composition of this salt is $\text{C}^6 \text{H}^8 \text{Ca}^2 \text{O}^6$. Its aqueous solution gives a white precipitate with nitrate of silver; this precipitate, decomposed by sulphuretted hydrogen, yields a liquid which contains a new acid, and which does not crystallize, but after evaporation forms a syrupy mass.

Comparing these acids with the polyethylenic alcohols from which they are formed, relations of a very simple character are found to exist between them, analogous to those which exist between alcohol and acetic acid; a certain quantity of hydrogen disappears and is replaced by an equivalent quantity of oxygen. When glycol is transformed into glycolic acid, we may assume that the radical ethylene, $\text{C}^2 \text{H}^4$, is changed into glycolyle, $\text{C}^2 \text{H}^2 \text{O}$; in the case of diethylenic alcohol, we may suppose that both the ethylene radicals are converted into glycolyle. Diethylenic alcohol thus becomes *diglycolic acid*. The acid formed from triethylenic alcohol is similarly derived; two ethylene radicals are converted into glycolyle, while the third remains intact. In this manner *diglycolethylenic acid* is obtained. The following formulæ exhibit these relations:—



Other acids may be formed from these polyethylenic alcohols. Those here described possess the molecular complication and the characters of the true vegetable acids, and they have been gradually built up by synthesis from olefiant gas.

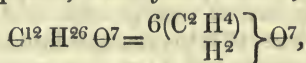
Some additional members have been added to the series of polyethylenic alcohols by Lourenço*. In his original method

* *Comptes Rendus*, September 3, 1860.

for preparing these bodies by heating glycol with bromide of ethylene, a liquid is formed containing, besides water and hydrobromic acid, a series of polyethylenic alcohols. By repeating this operation on a large scale, and by fractional distillation, he obtained diethylenic and triethylenic alcohols. The residue could not be distilled under the atmospheric pressure without decomposition. But by distilling it under a pressure of 0.025 millim., three compounds have been isolated. The first, *tetrethylenic alcohol*, $\text{C}^8 \text{H}^{18} \Theta^5 = \left. \begin{matrix} 4(\text{C}^2 \text{H}^4) \\ \text{H}^2 \end{matrix} \right\} \Theta^5$, boils at 230° under this pressure; it is identical in properties with the body obtained by Wurtz.

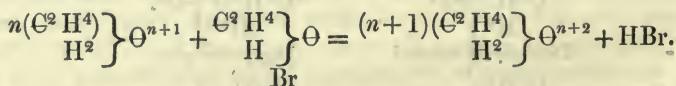
Pentethylenic alcohol, $\text{C}^{10} \text{H}^{22} \Theta^6 = \left. \begin{matrix} 5(\text{C}^2 \text{H}^4) \\ \text{H}^2 \end{matrix} \right\} \Theta^6$, boils at 281° under the pressure 0.025 millim. It is a viscous liquid resembling glycerine.

The third compound, *hexethylenic alcohol*,



boils at about 325° , and is distinguished from the former by its greater viscosity.

If the operation were conducted on a sufficiently large scale, the higher homologous compounds could be obtained. They have the general formula $\left. \begin{matrix} n(\text{C}^2 \text{H}^4) \\ \text{H}^2 \end{matrix} \right\} \Theta^{n+1}$. They are more viscous in proportion as their equivalent is higher, and there is a difference of about 45° between their boiling-points. Lourenço has examined the direct action of hydrobromic glycol on glycol, the lowest member of the series, and he represents the general equation for their formation in the following manner:—

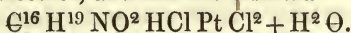


The hydrobromic acid formed reacts on the excess of glycol and regenerates the hydrobromic glycol, which in turn acts on the higher polyethylenic alcohols. Hence it is that no other bromine compound than hydrobromic glycol is formed, whatever be the stage at which the reaction is interrupted.

Anisic alcohol, $\text{C}^8 \text{H}^{10} \Theta^2$, contains twice as much oxygen as the ordinary alcohols; and it may be regarded either as an alcohol containing an oxygen radical, or as being analogous to the class of glycols. In the former case its formula would be written $\left. \begin{matrix} \text{C}^8 \text{H}^9 \Theta \\ \text{H} \end{matrix} \right\} \Theta$, and in the latter $\left. \begin{matrix} \text{C}^8 \text{H}^{8//} \\ \text{H}^2 \end{matrix} \right\} \Theta^2$. On the

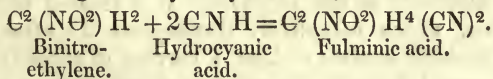
supposition that it is a glycol, cinnamene (styrol), $\text{C}^8 \text{H}^8$, would be its radical. Cannizaro is occupied with the question of the constitution of this body*. His researches are not yet finished; but in the meantime he publishes an account of two oxygenated alkalies which appear analogous to those obtained by Wurtz from oxide of ethylene.

When hydrochloric acid is passed into anisic alcohol, an ether is obtained which has the formula $\text{C}^8 \text{H}^9 \text{O} \text{Cl}$. It is analogous with chlorhydrine of glycol. When this body is treated for some time with strong alcoholic ammonia, a white deposit is obtained consisting of a mixture of sal-ammoniac and two organic bases. They are separated from sal-ammoniac by treatment with alcohol, and are separated from each other by solution in water and crystallization. The more soluble in water is a primary anisamine, $\text{C}^8 \text{H}^{11} \text{O} \text{N} = \left. \begin{array}{c} \text{C}^8 \text{H}^9 \text{O} \\ \text{H}^2 \end{array} \right\} \text{N}$, and the other is a secondary anisamine, $\text{C}^{16} \text{H}^{19} \text{O}^2 \text{N} = \left. \begin{array}{c} (\text{C}^8 \text{H}^9 \text{O})^2 \\ \text{H} \end{array} \right\} \text{N}$. The first one crystallizes in small needles, and the second in white plates. Both of them are strong bases. Both of them form compounds with bichloride of platinum. The compound of primary anisamine and bichloride crystallizes in small lustrous yellow plates of the formula $\text{C}^8 \text{H}^{11} \text{NO}$, HCl Pt Cl^2 . The other compound crystallizes in small yellow needles, and has the formula



In some recent researches on fulminic acid, Schischkoff† has been led to a formula for this acid different from that which he formerly proposed‡. He considers it to be $\text{C}^2 (\text{NO}^2)^2 \text{H}^4 (\text{CN})^2$, which represents alcohol in which the cyanogen replaces oxygen, and two equivalents of the group NO^2 two equivalents of hydrogen,—a formula which explains the formation of fulminic acid from alcohol.

The action of nitric acid on alcohol always gives rise to the formation of hydrocyanic acid; Schischkoff assumes that there is produced at the same time a binitro-ethylene, $\text{C}^2 (\text{NO}^2)^2 \text{H}^2$, which, combining with hydrocyanic acid, forms fulminic acid,



The reasons for this mode of viewing fulminic acid are,—

1. That the fulminates have the characteristic properties of the cyanides; treated with chlorine they yield chloride of cyanogen, and with hydrochloric acid they give hydrocyanic acid.

* *Comptes Rendus*, June 11, 1860.

† *Ibid.* July 16, 1860.

‡ *Phil. Mag.* vol. xiv. p. 100.

2. When the fulminates of silver or of mercury are treated with alkalies, a cyanate is formed,—a formation due to reduction of the nitro-compound under the influence of the affinity of the alkaline cyanides for oxygen.

3. The explosive properties of the fulminates would be readily explained by the excessive instability of binitro-ethylene.

4. Although the group binitro-ethylene is hypothetical, the discovery of the analogous bodies, nitroform, $\text{C}(\text{NO}^2)^3\text{H}$, and triacetoneitrile, $\text{C}^2(\text{NO}^2)^3\text{N}$, renders its existence by no means impossible; and its combination with hydrocyanic acid would be perfectly analogous to the combination of the hydracids with the hydrocarbons.

MM. Margueritte and Sourdeval* have made a series of researches on the formation of cyanogen, and the production of ammonia by means of atmospheric nitrogen. Their experiments have been made on a large scale, and they state—

1. That baryta, calcined in the presence of carbon and of atmospheric air, readily assimilates nitrogen and carbon, and that this *cyanuration* of barium is a very simple operation.

2. At a temperature of 300°C . cyanide of barium is decomposed by a current of aqueous vapour, and disengages the whole of the nitrogen which it contains under the form of ammonia. The industrial consequences of these two reactions are the manufacture of the cyanides of barium and potassium, of prussian blue, of ammonia, and of nitric acid and nitrates by known methods.

The authors add that the methods which they adopt for preparing baryta yield this base in such quantity that it may be practically used in the manufacture of sugar.

Von Hauer† has described some seleniates, and has also modified the methods of preparing selenic acid. Selenium is dissolved in nitric acid, and the selenious acid is converted into selenic acid by fusing with nitrate of soda. The mixture of seleniate and acetate of soda thus obtained is mixed with a solution of nitrate of lime. The seleniate of lime which forms is almost as insoluble as the sulphate. It is treated with oxalate of cadmium, which completely decomposes it: after filtration from the insoluble oxalate of lime, a current of sulphuretted hydrogen is passed through the solution of seleniate of cadmium. After filtering the precipitate of sulphuret of cadmium, the solution of selenic acid is freed by heating from excess of sulphuretted hydrogen.

* *Comptes Rendus*, June 11, 1860.

† *Sitzungsberichte der K. Academie der Wissenschaften zu Wien*, January 1860.

The following seleniates were prepared by dissolving the carbonates or hydrates in selenic acid :—

Seleniate of Soda, $\text{NaO}, \text{SeO}^3 + 10 \text{Aq.}$ —Large crystals resembling Glauber's salt.

Seleniate of Lime, $\text{CaO}, \text{SeO}^3 + 2 \text{Aq.}$ —Needles like sulphate of lime.

Seleniate of Nickel, $\text{NiO}, \text{SeO}^3 + 6 \text{Aq.}$ —These crystals resemble the corresponding sulphate, as has been shown by Mitscherlich. They lose four equivs. of water at 100° .

Seleniate of Nickel and Potassium, $\text{KO}, \text{NiO}, 2\text{SeO}^3 + 6 \text{Aq.}$ —Obtained by the spontaneous evaporation of a solution of the two salts. They lose four equivs. of water at 100° . The analogous sulphate is not perceptibly altered at 100° .

Seleniate of Cadmium, $\text{CdO}, \text{SeO}^3 + 2 \text{Aq.}$ —These crystals differ both in form and composition from those of the corresponding sulphate. They lose 1 equiv. of water at 100° .

Weber has described the formation of oxychloride of selenium and of the alum of selenic acid*. The former body is obtained by distilling chloride of selenium, SeCl^2 , so that it comes in contact with an equal volume of selenious acid. The two bodies combine to form white vapours of oxychloride of selenium, which is redistilled over selenious acid.

It is a yellowish liquid which fumes in moist air; it has the density 2.44, and boils at about 240° . It dissolves readily in water, forming hydrochloric acid and selenious acid. Its composition may be expressed by the formula $\text{SeCl}^2 + \text{SeO}^2$, according to which it is a compound of selenious acid and of chloride of selenium. It may also be considered as being selenious acid in which an atom of oxygen is replaced by an atom of chlorine. Oxychloride of selenium is also formed by the action of a small quantity of water on chloride of selenium.

It was found impossible to prepare a compound corresponding to Regnault's chlorosulphuric acid. Chlorine is without action on dry selenious acid.

To prepare selenic acid, selenite of soda was fused with nitrate of potass, the mixture treated with nitrate of lead, and the precipitate of seleniate of lead decomposed by sulphuretted hydrogen. The solution of selenic acid was concentrated, and a quarter of it neutralized with carbonate of potass; the remaining three quarters were digested with pure hydrate of alumina. The mixed liquids were left to spontaneous evaporation, and after the expiration of twenty days, beautiful crystals were obtained. Their composition was $\text{Al}^2 \text{O}^3, 3\text{SeO}^3, \text{KO}, \text{SeO}^3 + 24 \text{HO}$. The crystals

* Poggendorff's *Annalen*, vol. cviii. p. 613.

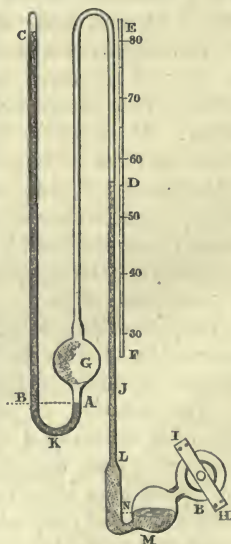
have just the form of those of ordinary alum ; they are combinations of the octahedron and cube. They have all the lustre of alum crystals, than which they are somewhat more soluble. When heated, they give off water and finally selenious acid and oxygen.

XXXVII. *On the Construction of a new Air-thermometer.*

By THOMAS TATE, Esq.*

AIR, from the uniformity and extreme range of its expansion under an increase of temperature, seems to be the substance best fitted for measuring changes of temperature. But the indications of the ordinary air-thermometer at present in use are subject to derangement from the varying pressure of the atmosphere. In the differential thermometer, it is true, the atmospheric pressure is altogether excluded ; but then this instrument cannot be employed, as in the case of the common thermometer, for indicating the temperature of the surrounding air. Now in the instrument here proposed (represented in the annexed diagram), the variations of pressure of the atmosphere are corrected by an easy adjustment ; and the air in the instrument being thus maintained at a constant pressure, the volume of this air becomes a true measure of the change of temperature.

G and N are small glass globes, about an inch and a tenth in diameter, connected with the bent tubes, as shown in the diagram ; A K C a barometer-tube about 20 inches in length ; E L a wide thermometer-tube about $\frac{1}{11}$ th of an inch diameter ; L N M a tube, somewhat larger in diameter, connected with the globe N, which terminates with an open tube ; B an india-rubber ball, about $1\frac{1}{2}$ inch diameter, having an open tube proceeding from it, forming an air-tight connexion with the globe N ; S the head of an adjustment-screw, which, working in a hollow screw formed in the cross bar H I, compresses the ball B as may be required ; A B C and M L D are mercury columns ; the globe G and tube A E D contain dry air, and the space above the mercury C is a Torricellian vacuum, so that the



* Communicated by the Author.

column of mercury BC is a constant measure of the pressure of the air in the globe G ; EF a scale of degrees, the graduation of which will be hereafter described. A mark is placed at A , so that the air in the globe may be always brought to a constant pressure by means of the adjustment screw S . Suppose the temperature of the air to change, then a slight change will be observed in the level of the mercury at A ; by relaxing or tightening the screw S , as the case may require, the mercury is brought on a level with the mark A (which may be done with mathematical precision), and then the number on the scale EF coincident with the surface of the mercury in the thermometer-tube will give the temperature of the surrounding air. The scale EF is graduated in the following manner:—

The tube AKC is filled with mercury in the same manner as that of an ordinary barometer. The globes and tubes being filled with dry air (by a process not necessary to describe here), and a sufficient quantity of mercury being introduced at M , the air is exhausted (by means of an air-pump attached to the tube of the globe N) until the mercury falls for a short space in the tube BC ; then after allowing the external air to enter the globe N , the mercury will rise in the tube ME , say to J , so that the sum of the columns BC and NJ will be equal to the column of mercury balancing the pressure of the atmosphere at the time; that is to say, if this column be 30 inches, and BC be 18 inches, then NJ will be 12 inches. The india-rubber ball, in a somewhat flaccid state, being attached to the globe N , and the screw bar $I H$ fixed, the screw-head S is turned so as to compress the air in the ball until the mercury in the tube ME stands at D about the middle of the tube EJ when the air is at or near to mean temperature; a mark is then made at A coincident with the level of the mercury. The ball of a delicate standard thermometer being placed beside the ball G , two extreme temperatures, t and t_1 , are noted, and marks placed coincident with the corresponding levels of mercury in the tube LE ; then the space between these marks will give a reading of degrees equal to the difference between the two temperatures; hence the scale is readily formed.

Let V = the volume of the air at t temperature.

P = its corresponding pressure measured by the column of mercury BC .

p = the length of the column of AKB .

a = section of the tube D .

b = section of the tube ABC .

V_1 = the volume of the air at t_1 temperature, P_1 being its corresponding pressure.

L_{t_1-t} = change of the column of mercury D corresponding to t_1-t difference of temperature.

$\frac{1}{\alpha}, \frac{1}{\rho}$ = the expansion of mercury and glass respectively per unit of volume, for t_1-t degrees change of temperature.

Then we have

$$V_1 \cdot \frac{P_1}{P} - V = \frac{V}{458+t} (t_1-t). \quad . \quad . \quad . \quad (1)$$

When the expansion of the mercury and glass are neglected, $P_1=P$, and in this case equation (1) becomes

$$V_1 - V = \frac{V}{458+t} (t_1-t);$$

but

$$V_1 - V = aL_{t_1-t},$$

$$\therefore L_{t_1-t} = \frac{V}{a(458+t)} (t_1-t), \quad . \quad . \quad . \quad (2)$$

which shows in this case that L_{t_1-t} is proportional to t_1-t , and consequently that the graduations on the scale EF must be uniform.

From equation (2), we have

$$\text{Length of 1 degree on the scale} = \frac{V}{a} \cdot \frac{1}{458+t},$$

and

$$\text{Range for T degrees} \quad . \quad . \quad . \quad = \frac{V}{a} \cdot \frac{T}{458+t}.$$

If $V=1$, $a=.006$, $t=50$, and $T=54$; then we find the length of one degree on the scale to be about one-third of an inch, and the range or length of the whole scale EF to be about 18 inches.

It will be observed that the glass ball G need not greatly exceed the size of the bulbs of some of our ordinary thermometers; and the whole length of the instrument may be 5 or 6 inches less than that of a barometer.

These formulæ are sufficiently exact for determining the proportion of the different parts of the instrument; but it is necessary that we should determine the amount of the disturbances arising from the expansion of the mercury and glass.

Throughout the following investigation, all terms are neglected which contain as a factor the product of $\frac{1}{\alpha}$ and $\frac{1}{\rho}$, or the square of either of them.

Let z = the increment of the column BC due to the change of temperature; then

Volume of mercury in ABC before expansion

$$= (P + p)b.$$

Volume of this mercury after its expansion

$$= (P + p)b \left(1 + \frac{1}{\alpha}\right). \quad \dots \dots \dots (3)$$

Section of the tube after its expansion

$$= b \left(1 + \frac{2}{3\rho}\right).$$

Volume of mercury in ABC after its expansion

$$= (P + p + z)b \left(1 + \frac{2}{3\rho}\right). \quad \dots \dots \dots (4)$$

Therefore, by equating (3) and (4), we get

$$P + z = \frac{(P + p) \left(1 + \frac{1}{\alpha}\right)}{1 + \frac{2}{3\rho}} - p;$$

but

$$P_1 \left(1 + \frac{1}{\alpha}\right) = P + z;$$

$$\begin{aligned} \therefore \frac{P_1}{P} &= \left(1 + \frac{p}{P}\right) \frac{1}{1 + \frac{2}{3\rho}} - \frac{p}{P} \cdot \frac{1}{1 + \frac{1}{\alpha}} \\ &= \left(1 + \frac{p}{P}\right) \left(1 - \frac{2}{3\rho}\right) - \frac{p}{P} \left(1 - \frac{1}{\alpha}\right). \end{aligned}$$

On the supposition that the marks corresponding to the temperatures t and t_1 are made upon the glass, V being the volume of the air at t temperature; then we have

$$V_1 = V \left(1 + \frac{1}{\alpha}\right) + L_{t_1-t} \times a \left(1 + \frac{2}{3\rho}\right).$$

Let $\frac{1}{\alpha'}$ and $\frac{1}{\rho'}$ be put for the expansions for one degree, then

$$\frac{1}{\alpha} = \frac{t_1 - t}{\alpha'}, \text{ and } \frac{1}{\rho} = \frac{t_1 - t}{\rho'}.$$

Substituting the values of $\frac{P_1}{P}$ and V_1 in equation (1), and solv-

ing the resulting equality for L_{t_1-t} , we get

$$L_{t_1-t} = \frac{V}{a} (t_1-t) \left\{ \frac{1}{458+t} - \frac{1}{3\rho'} - \frac{p}{P} \left(\frac{1}{\alpha'} - \frac{2}{3\rho'} \right) \right\} \\ \times \left\{ 1 - (t_1-t) \frac{p}{P} \left(\frac{1}{\alpha'} - \frac{2}{3\rho'} \right) \right\}. \quad (5)$$

Neglecting $(t_1-t) \frac{p}{P} \left(\frac{1}{\alpha'} - \frac{2}{3\rho'} \right)$ as being very small compared with 1, we have

$$L_{t_1-t} = \frac{V}{a} (t_1-t) \left\{ \frac{1}{458+t} - \frac{1}{3\rho'} - \frac{p}{P} \left(\frac{1}{\alpha'} - \frac{2}{3\rho'} \right) \right\}, \quad (6)$$

where L_{t_1-t} is proportional to t_1-t . This expression is an exceedingly near approximation to the true formula.

Let the space L_{t_1-t} , expressed by equation (5), be divided into t_1-t equal parts, then each part will be the length of one degree on the scale, that is,

$$\text{Length of 1 degree on the scale} = L_{t_1-t} \times \frac{1}{t_1-t};$$

$$\therefore \text{Length of } t_2 \text{ degrees on the scale} = L_{t_1-t} \times \frac{t_2}{t_1-t}.$$

But the *true* length of a degree will be derived from equation (5) by making $t_1-t=1$, that is,

$$\text{True length of 1 degree} = L_1, \text{ and}$$

$$\text{True length of } t_2 \text{ degrees} = L_{t_2};$$

$$\therefore \text{Error in } t_2 \text{ degrees} = L_{t_2} - L_{t_1-t} \times \frac{t_2}{t_1-t}.$$

Putting x' = the error in t_2 degrees taken as a proportional part of the length of one degree on the scale; then

$$x' = \frac{L_{t_2} - L_{t_1-t} \times \frac{t_2}{t_1-t}}{L_{t_1-t} \times \frac{1}{t_1-t}} \\ = t_2(t_1-t-t_2) \frac{p}{P} \left(\frac{1}{\alpha'} - \frac{2}{3\rho'} \right). \quad (7)$$

This expression gives the correction for any temperature $t+t_2$ or $t-t_2$, that is, for any temperature t_2 degrees above or below t .

The only dimensions of the instrument involved in this expression are the lengths of the columns A K B and B C.

When t_2 is less than $t_1 - t$, the value of x' is positive; in this case, therefore, the value of x' must be added to the temperature indicated by the scale.

When t_2 is minus, or when t_2 is greater than $t_1 - t$, then in both cases the value of x' is negative; in these cases, therefore, the value of x' must be subtracted from the temperature indicated by the scale. In both these cases the value of x' increases with t_2 .

If t and t_1 be taken as the extreme temperatures of the instrument, then we have as the condition of maximum error,

$$\frac{dx'}{dt_2} = 0;$$

whence we find

$$\text{Max. value of } x' = \left(\frac{t_1 - t}{2}\right)^2 \frac{p}{P} \left(\frac{1}{\alpha'} - \frac{2}{3\rho'}\right). \quad (8)$$

$$\text{Let } t = 32^\circ, t_1 = 86^\circ, \frac{p}{P} = \frac{1}{6}, \frac{1}{\alpha'} = \frac{1}{18 \times 550}, \frac{1}{\rho'} = \frac{1}{18 \times 4160};$$

then we find max. value of $x = .01$; that is, in this case the maximum error would be the hundredth part of a degree.

But as it would be more convenient for construction to take t and t_1 at certain mean limits, thus, let $3(t_1 - t)$ be the extreme range of temperature, that is, $t_1 - t$ degrees above t_1 , and $t_1 - t$ degrees below t ; then we find, by substituting $+2(t_1 - t)$ or $-(t_1 - t)$ for t_2 in equation (7),

$$\text{Max. value of } x' = -2(t_1 - t)^2 \frac{p}{P} \left(\frac{1}{\alpha'} - \frac{2}{3\rho'}\right). \quad (9)$$

Let $t = 50^\circ, t_1 = 68^\circ$, giving an extreme range of temperature between 32° and 86° ; then taking the values of the other quantities the same as before, we find the

$$\text{Max. value of } x' = .0096;$$

that is, in this case the maximum error would be less than the hundredth part of a degree,—an almost inappreciable quantity.

Within this range of temperature, therefore, the indications of the instrument may be taken as perfectly true.

Hastings, September 14, 1860.

XXXVIII. *On the presence of Arsenic and Antimony in the sources and beds of Streams and Rivers.* By DUGALD CAMPBELL, *Analytical Chemist to the Hospital for Consumption, Brompton**.

SOME months ago I had a specimen of iron pyrites from coal, or coal pyrites as it is familiarly called, sent to me, to examine for arsenic, and I was somewhat surprised to find in it the quantity of arsenic which I did, arsenic not being generally set forth in the analysis which I have seen of such pyrites, although my own impression was that, if looked for in a proper manner, it would be found.

Having been informed that the coal associated with this pyrites crops out in the beds of the streams and rivers of the district from which my specimen of coal pyrites was sent, it occurred to me that if the sand from the beds of such streams or rivers were carefully examined, arsenic would be found in it.

Having obtained a specimen of the sand, after a great many experiments (for I was at first beset with difficulties about the purity of the various materials I used in my examinations, and other causes) I at last succeeded in obtaining arsenic from it in every experiment, and really in notable quantity.

Besides arsenic, I also got antimony, which I had not observed in my coal pyrites, although it may very likely have been there in small quantity.

The quantity, however, of both the arsenic and the antimony appearing so considerable in this specimen of sand, induced me to think that their occurrence was due to other sources besides the coal pyrites, and led me to the opinion that I should find these metals in the sand from the sources or beds of most streams or rivers in greater or less quantity, depending upon the geological formation from which such sand had its origin; and after making several experiments, I found my views confirmed in this respect; and in every specimen of sand from these sources which I have hitherto examined, I have obtained arsenic, and generally also antimony, and, as I had supposed, in some instances the arsenic was in considerable quantity, in others in small quantity; but in no instance is its presence in a doubtful quantity.

I say "generally also antimony," although it is more than likely that antimony exists in every specimen of sand which I have examined; but the process I followed was especially for arsenic, not for antimony, although, as I have stated, I very often got some antimony at the same time with the arsenic.

Although I speak of the arsenic being in greater quantity in the one sand than in the other, this is observed as yet only by

* Communicated by the Author.

qualitative tests. I have not directly estimated quantitatively the arsenic in any one instance; nor do I think that by the process which I follow to detect the arsenic, and which I shall give, should I get, in most instances, if not in all, anything like the quantity which the sand contains.

The process which I adopt is as follows:—The sand is first dried by means of a bath at not too high a temperature, and sifted, if necessary, through a coarse sieve, to remove anything larger than sand; the portion for the experiment I then weigh out. I seldom require more than two ounces of the dry sifted sand. I then ascertain that my hydrochloric acid is free from arsenic (or rather how far it is contaminated with it, for it is rarely indeed that this acid is found entirely free from arsenic), which I do by introducing into a drachm of the acid diluted with about three drachms of distilled water, when brought to boil in a small flat-bottomed flask, about the eighth of a square inch of bright copper-foil, specially prepared as free from arsenic as it is possible to be obtained*, and gently boiling the same from about half an hour to three-quarters or so, avoiding much evaporation by having a small funnel in the mouth of the flask. Should this piece of copper get coated with arsenic, I take it out and replace it by a similar piece, and boil it in a similar manner; should this piece likewise get coated, I should scarcely like to use such impure acid; but if no coating has taken place, or only one piece was coated, I measure out into a small flat-bottomed flask (a flask capable of holding about three liquid ounces is the size I prefer), half an ounce of the acid, and gradually add to it the two ounces of the dry sifted sand. I now close the bottle with a cork, and allow it to stand for some hours until the acid has thoroughly penetrated the sand, when I remove the cork, and at once attach, by means of a perforated cork, a bent tube about 3 feet long, leading into a receiver through a cork, a little loose at first, but capable of being made tight afterwards, the receiver being placed in water. Heat is now applied by means of a sand-bath, gently at first, gradually increasing when the cork at the receiver is made to fit tight. I also apply wet rags or bibulous paper down the tube. In about an hour the greater part of the distillate will have passed into the receiver, when it may be detached. The distillate, which varies in quantity according to the nature of the sand, I measure. I have found it to average 190 grains; but, as stated before, it varies in quantity according to the nature of the sand.

The test which I generally first apply is the copper one,

* I have lately examined a great many specimens of copper said to be free from arsenic; of these, eight specimens were electrotypes, but in all I found arsenic.

exactly as I have described in testing the acid, only, instead of a drachm, I use a less quantity, proportionate to the acid used and the distillate obtained. After one piece of copper is coated, it is withdrawn and another substituted, and so on until no more coating takes place; much evaporation of the liquid is made up from time to time by a little boiling water.

When antimony is in the distillate, it deposits more readily than the arsenic upon the copper, and the first pieces coated have more of a purple or blue tint than the steel-grey colour of the arsenic.

The remainder of the distillate may be tested by a Marsh's apparatus, heating the gas as it passes through a quill-tube drawn to a fine point, in order to get the metallic arsenic deposited upon the tube.

The metallic arsenic, deposited in this way from the residue of the distillate from two ounces of the sand, is in many instances of sufficient quantity to dissolve or sublime and examine by other tests.

The sands which I have examined have been selected with much care; and in most cases, when taken from the beds of streams or rivers, they have been taken above where they could receive contamination from coal-pits or works, or contamination of any description whatever: I append a list of them, in order that any one may repeat the experiments.

I am indebted to Mr. Woodcroft, of the Great Seal Patent Office, for the first eight specimens, and to Mr. George Phillips, of the Inland Revenue Laboratories, for the next four specimens, both these gentlemen having taken much interest and trouble in the matter. For most of the other specimens I am indebted to kind friends, and one or two I have procured myself.

List of Sands examined.

From Cragg Hall, Cheshire.

From Black Brook, which falls into the Dane, Staffordshire.

From the source of the Wye, the Buxton Water supply.

From the source of the Goyte, Derbyshire.

From Oaks Clough Redmires, supply of Sheffield.

From the source of the River Porter, Fulwood, Sheffield.

From Hog Shaw Brook, Buxton.

From the source of the River Dove, Derbyshire, where it issues from a compact limestone rock at Mr. Marsden's Dowal.

From Middle Lake, Dinish Island, Killarney, Ireland, County Kerry.

From Flesk River.

From Flesk River, Killarney, Ireland, County Kerry.

From Ogiven River, near Penrhyn Slate Quarries, about four or

five miles from the outlet of Ogiven Lake, Carnarvonshire, North Wales.

From Snowdon, 100 yards above the Ceunant Maur Fall, Carnarvonshire, North Wales.

From the junction of the River Kinder and Sett, Derbyshire.

From the Brent, two miles below Apperton Bridge,

From the Thames, a quarter of a mile above Teddington lock.

From the Mardyke, near Stifford, Essex; falls into the River Thames at Grays.

From the River Lea, Essex, 300 yards above the East London Water Works.

From brook emptying itself into the Wrike, Melton Mowbray, Leicestershire.

From Ladies Well Spring, Pitlour, Fifeshire, Scotland.

From Garden Brook, Pitlour, Fifeshire, Scotland.

Although in my experiments I have not yet met with a sand from the source or bed of a stream or river that does not contain arsenic, and perhaps antimony also, yet, judging from the exceeding variability in the quantity in each, I am far from thinking that sands will not be found, though I should say they will be few in number, that will not contain these metals.

XXXIX. *Meditation on the Idea of Poncelet's Theorem.* By J. J. SYLVESTER, M.A., F.R.S., Professor of Mathematics in the Royal Military Academy, Woolwich*.

HITHERTO Poncelet's theorem has been regarded as a method *sui generis* and complete in itself; but in truth it is but the first germ or rudiment of a vast and prolific algebraical theory; and not only so, but the principle which it contains admits of applications of the utmost value in various dynamical and analytical questions, which it is surprising should have been allowed to lay so long dormant. For the present, however, I mean to confine myself to a very brief indication of one direction in which the theorem admits of being generalized. And first I will make a remark upon so simple a matter as the extraction of the square root, which seems to have escaped observation, and at all events is so far from being generally known, that two of the highest authorities for mathematical erudition in this country whom I have consulted on the subject provisionally accept it as new.

Let r be an approximate value of \sqrt{N} ; then by that mode of application of Newton's method of approximation to the equation $x^2 = N$ which is equivalent to the use of continued fractions,

* Communicated by the Author.

we may easily establish the following theorem, viz. that

$$\frac{r^2 + N}{2r}, \quad \frac{r^3 + 3rN}{3r^2 + N}, \quad \frac{r^4 + 6r^2N + N^2}{4r^3 + 4rN}, \quad \frac{r^5 + 10r^3N + 5rN^2}{5r^4 + 10r^2N + N^2}, \dots^*$$

will be successive approximations to \sqrt{N} , whose limits of error can be assigned when a limit to the error of the first approximation r is given. The coefficients of the q th approximation, it will be observed, are for the numerator the alternate binomial coefficients

$$1, \quad q \frac{q-1}{2}, \quad q \cdot \frac{(q-1)}{2} \cdot \frac{(q-2)}{3} \cdot \frac{q-3}{4}, \text{ \&c.};$$

and for the denominator the intermediate ones,

$$q, \quad q \cdot \frac{q-1}{2} \cdot \frac{q-2}{3}, \text{ \&c.}$$

Mr. Cayley has reminded me that the third approximation, $\frac{r^3 + 3rN}{3r^2 + N}$, is a special case of a formula for *any* root of N given in the books; and to Mr. De Morgan I am indebted for a hint which has led me to notice that all these forms may be deduced from the Newtonian method of approximation†.

If we call the i th approximation $\phi(i, r)$, we shall find that the functional equation $\phi(j, \phi(i, r)) = \phi(ij, r)$ will be satisfied; which is not so mere a truism as might at first sight be supposed, as any one may satisfy himself by studying the analogous theory for cubic or higher roots, a part of the subject to which I may hereafter return.

Now as to the limits of accuracy afforded by the successive approximations. Let e be a known limit to the relative error of the first approximation r , by which I mean that $\left(\frac{\sqrt{N} - r}{\sqrt{N}}\right)^2 < e^2$.

* In other words, if r be the first approximation to \sqrt{N} , the i th approximation will be

$$\frac{(r + \sqrt{N})^i + (r - \sqrt{N})^i}{(r + \sqrt{N})^i - (r - \sqrt{N})^i} \sqrt{N},$$

so that the relative error becomes

$$\frac{2(r - \sqrt{N})^i}{(r + \sqrt{N})^i - (r - \sqrt{N})^i},$$

in which form the theorem is self-subsistent, and needs no proof. But the fact remains interesting, that the application of Newton's method of approximation to the equation $x^2 = N$ will be found to lead to the form above written at the i th step of the process conducted after the continued-fraction fashion.

† The expansion (after Newton) of \sqrt{N} introduces the binomial coefficients—a curious fact! What are the analogous integers which the continued-fraction process applied to $\sqrt[3]{N}$ will produce?

For greater simplicity, I take separately the cases where r is too great and r is too small.

1. Let $\sqrt{N} < r < (1 + \epsilon) \sqrt{N}$; then the errors will be throughout in excess; and we may assign as a limit of error to the i th approximation a quantity, say ϵ_i , which is a known function of ϵ , viz. $\frac{2}{(2\epsilon^{-1} + 1)^i - 1}$, which it may be noticed is less than $\frac{\epsilon^i}{2^{i-1}}$.

2. Let $\sqrt{N} > r > (1 - \eta) \sqrt{N}$; then the errors will be alternately in defect and excess, and to the i th approximation we may assign a limit of error η_i , where $\eta_i = \frac{2}{(2\eta^{-1} - 1)^i - (-1)^i}$.*

We may now apply these results to Poncelet's linear approxi-

* If we write

$$\epsilon_i = \theta(\epsilon, i) \text{ and } \eta_i = \vartheta(\eta, i),$$

then if i be any *odd* number,

$$\theta(\theta(\epsilon, i), j) = \theta(\epsilon, ij),$$

$$\vartheta(\vartheta(\eta, i), j) = \vartheta(\eta, ij);$$

and if i be any *even* number,

$$\vartheta(\theta(\epsilon, i), j) = \theta(\epsilon, ij),$$

$$\theta(\vartheta(\eta, i), j) = \vartheta(\eta, ij).$$

Or more simply, if the error in excess be treated as positive, and in defect as negative, and δ_i the i th limit of error, we shall have

$$\delta_i = \frac{2(-)^{i-1}\delta^i}{(2+\delta)^i - \delta^i};$$

and calling $\delta_i = \theta(i, \delta)$,

$$\theta(j, \theta(i, \delta)) = \theta(ij, \delta).$$

Thus, then, if we call $\frac{N+x^2}{2x} = \psi x$, $\psi^q x$ will correspond to the (2^q) th order of approximation, and the absolute value of the error will be less than

$$\frac{2\delta^{2^q}}{(2+\delta)^{2^q} - \delta^{2^q}}.$$

By way of example, suppose we take 6 as our first approximation to $\sqrt{31}$, then

$$\delta < \frac{\frac{1}{2}}{5\frac{1}{2}} < \frac{1}{11};$$

and if we make $\psi x = \frac{31+x^2}{2x}$, we shall have

$$\psi^4 6 : \sqrt{31} :: 1 + \omega : 1,$$

where

$$\omega < \frac{2}{23^{16} - 1},$$

which serves to exemplify the prodigious rapidity of the approximation in this method of extracting the square roots of numbers.

mate representation of $\sqrt{a+bx+cx^2}$. Suppose $f+gx$ is the first approximation, as found by Poncelet's method, with a maximum relative error e , then $\frac{(f+gx)^2 + (a+bx+cx^2)}{2(f+gx)}$ will be a much closer approximation, with a relative error never exceeding $\frac{e^2}{2+e}$ in excess, nor $\frac{e^2}{2-e}$ in defect. So a still nearer approximation will be $\frac{(f+gx)^3 + 3(f+gx)(a+bx+cx^2)}{3(f+gx)^2 + a+bx+cx^2}$, with a relative error never exceeding $\frac{e^3}{4+6e+3e^2}$ in excess, nor $\frac{e^3}{4-6e+3e^2}$ in defect, and so on. The marvellous facility which these formulæ afford for the calculation of elliptic and ultra-elliptic functions, and not merely for their computation as by a method of quadratures, but (which is of far greater importance) their quasi-representation under circular and logarithmic forms, with assignable limits of proportional error, will be illustrated in a future communication. As regards the idea of substituting rational for irrational functions, I have only to-day learned from Mr. Cayley that I am anticipated in this by Mr. Merrifield*, in a paper very recently read before the Royal Society, but not yet printed in the Transactions†.

* I quite concur with Mr. Merrifield, and in fact before being made acquainted with the existence of his paper, had emitted the same opinion (among others to Dr. Borchardt of Berlin), that the substitutive method, consisting in the employment of rational functions in place of the radical, affords by far the most expeditious means for the calculation of elliptic functions of all orders, especially the third, and supersedes the necessity for the construction of special auxiliary tables. I believe, however, that my substitutions, founded on Poncelet's views, are in general the best that can be employed for the purpose. In addition to other advantages they possess this, which deserves notice—that as we know *à priori* a superior limit to the proportional error, the arithmetical values of the integrals to which they are applied may be brought out correct to any required place of decimals, without its being necessary to calculate and compare a superior and inferior limit to the integral, either one of these being sufficient in my method to indicate its own reliable degree of precision.

† In general it is obvious, if ϕx between the limits a and b retain always the same sign, and ψx within these limits be sometimes greater and sometimes less than ϕx , but the difference between them be always less than $\epsilon \phi x$, then $\int_b^a dx \psi x$ will differ from $\int_b^a dx \phi x$ by *considerably* less than $\epsilon \int_b^a dx \phi x$. Paradoxical, however, as it may at first sight appear, there are extreme cases where this difference tends to a ratio of equality with $\epsilon \int_b^a dx \phi x$. The complete elliptic function of the first order may be made

The method, however, of Mr. Merrifield in working out this conception is, I believe, entirely different from that here indicated: how the many mathematicians of a practical stamp, English and foreign, who have worked with Poncelet's method during the last quarter of a century, should have managed to

to furnish an example of this. Let

$$\phi x = \frac{1}{\sqrt{(1-x^2)(1-c^2x^2)}} = \frac{\sqrt{(1-x^2)+b^2x^2}}{\sqrt{1-x^2}(1-c^2x^2)}$$

(so that $b^2 = 1 - c^2$), and let

$$\psi x = \frac{f\sqrt{1-x^2} + gbx}{\sqrt{1-x^2}(1-c^2x^2)};$$

if we make

$$f = \frac{2}{1+\sqrt{2}}, \quad g = \frac{2}{1+\sqrt{2}}, \quad \epsilon = \frac{\sqrt{2}-1}{\sqrt{2}+1},$$

it follows from Poncelet's theorem, that for all values of x intermediate between 0 and 1, ψx will differ from ϕx by less than $\epsilon \phi x$.

Now it will easily be found by ordinary integration that

$$\int_0^1 dx \psi x = \frac{f}{2c} \log \frac{1+c}{1-c} + \frac{g}{c} \tan^{-1} \frac{b}{c}.$$

Hence $\int_0^1 dx \phi x$ must be always less than

$$\frac{f}{2(1-\epsilon)c} \log \frac{1+c}{1-c} + \frac{g}{(1-\epsilon)c} \tan^{-1} \frac{b}{c},$$

$$i. e. < \frac{1}{2c} \log \frac{1+c}{1-c} + \frac{1}{c} \tan^{-1} \frac{b}{c},$$

when c becomes indefinitely near to unity; that is, when b becomes indefinitely small, this approaches indefinitely near to $\log \frac{2}{b} + \frac{\pi}{2}$. But we know, by a theorem of Legendre, that the approximate value for the integral in such case is $\log \frac{4}{b}$; so that the superior limiting value of $\int_0^1 dx \phi x$,

found by the application of Poncelet's method, approaches in this instance indefinitely near to the value itself. The explanation of this is easy. As c approximates to unity, the only important values of x in the integral

$$\int_0^1 \frac{dx}{\sqrt{(1-x^2)(1-c^2x^2)}}$$

are those which lie in the *immediate* vicinity of 1; and for all such values the relative error is at a *negative maximum*.

It is not a little remarkable that so rude an application of Poncelet's method should serve to indicate almost with the force of rigorous demonstration the approximate formula $F(c) = \log \frac{1}{b} + \text{constant}$, when c ap-

overlook so obvious and important an extension of the principle and its applications, I find hard to realize; and my wonder is even greater that I should not have been anticipated twenty years ago, than that I should have been anticipated so recently. But the algebraical theory to which this extension points the way is replete with interest of a far higher order than its applications to practice; for plainly the derived approximate fractions, however sufficient for the purposes of computation, are

proaches indefinitely near to unity, the constant left undetermined being known to be less than $\log 2 + \frac{\pi}{2}$.

Nay, the demonstration may be made absolutely rigid if we set about to find an inferior limit. To this end make

$$\psi x = \frac{1}{\sqrt{1-x^2}(f\sqrt{1-x^2}+gbx)},$$

we shall find without difficulty

$$\int_0^1 \psi dx = \frac{1}{f\gamma} \log \frac{1+\gamma-b}{\gamma-1+b} \cdot \frac{\gamma+b}{\gamma-b}, \text{ where } \gamma = \sqrt{1+b^2},$$

and consequently we shall obtain as an *inferior* limit to $F(c)$ the expression

$$\frac{1}{\gamma} \log \frac{1+\gamma-b}{\gamma-1+b} \cdot \frac{\gamma+b}{\gamma-b},$$

which approaches indefinitely near to $\log \frac{2}{b}$ as c approaches indefinitely near to unity. It is thus seen that Legendre's $F(c)$, when c is indefinitely near to 1, lies between $\log \frac{2}{b}$ and $\log \frac{2}{b} + \frac{\pi}{2}$; the arithmetical mean between these limits is $\log \frac{2}{b} + \frac{\pi}{4}$, i. e. $\log \frac{1}{b} + 1.4785$, differing by only .0923 from the true value $\log \frac{1}{b} + \log 4$. Of course, when the form of $F(c)$ in the case supposed is known, viz. $\log \frac{1}{b} + C$, there is no difficulty in determining C (as may be seen in Verhulst's *Traité de Fonctions Elliptiques*); but the process above given of throwing the general value of $F(c)$ between limits, is, I believe, by far the easiest and most natural method of obtaining this form. The limits themselves, it should be noticed, have virtually been found by the method, simple to naïveté, of writing $\sqrt{1-c^2x^2} = \sqrt{p^2+q^2}$, where $p = \sqrt{1-x^2}$ and $q = bx$, and then substituting for $\frac{1}{\sqrt{p^2+q^2}}$, $\frac{1}{p+q}$ as an inferior, and $\frac{p+q}{p^2+q^2}$ as a superior limit in the quantity to be integrated. Closer and calculable limits *ad libitum* to the integral may be arrived at by substituting for $\frac{1}{\sqrt{p^2+q^2}}$ one or the other of the two following rational functions of p, q , according as we wish to obtain an inferior or superior

not, nor ever can be the best and closest of their respective kinds*. To fix the ideas, let us confine ourself to the *second* Ponceletic approximation to $\sqrt{a+bx+cx^2}$, viz. that which has the form $\frac{\lambda+\mu x+\nu x^2}{1+qx}$, where λ, μ, ν are to be determined. The problem to be solved is the following.

$$\begin{aligned}\text{Let} \quad \lambda + \mu x + \nu x^2 &= V, \\ (1+qx) \sqrt{a+bx+cx^2} &= U;\end{aligned}$$

it is required to assign the four constants λ, μ, ν, q , so that the

limit to the integral, viz.

$$\frac{(p+q+\sqrt{p^2+q^2})^i - (p+q-\sqrt{p^2+q^2})^i}{(p+q+\sqrt{p^2+q^2})^i + (p+q-\sqrt{p^2+q^2})^i} \cdot \frac{1}{\sqrt{p^2+q^2}},$$

or

$$\frac{(p+q+\sqrt{p^2+q^2})^i + (p+q-\sqrt{p^2+q^2})^i}{(p+q+\sqrt{p^2+q^2})^i - (p+q-\sqrt{p^2+q^2})^i} \cdot \frac{1}{\sqrt{p^2+q^2}},$$

in which formulæ the greater i is taken the closer will be the approximation. I am not aware that any of these limits to $F(c)$ (even the simplest of which, viz. those given above, may have some value for computational purposes, and have fallen thus very incidentally in my way) have ever before been noticed.

It is not unworthy of notice that the second superior limit to $\frac{1}{\sqrt{p^2+q^2}}$, viz. $\frac{p^2+pq+q^2}{(p+q)(p^2+q^2)}$, is an arithmetic mean between the first superior and first inferior limits, and consequently our second superior limit to the integral when b is indefinitely small becomes $\log \frac{2}{b} + \frac{\pi}{4}$, which brings the constant much nearer to its true value than did the use of the first limit; and as this approximation will evidently not stop at the second step of the process, we may safely infer that the integral derived from either formula when $i=\infty$ (for all values of b , whether finite or indefinitely small), not merely bears to $F(c)$ a ratio differing infinitely little from that of equality, but is absolutely equal to, and may for all analytical purposes be employed to represent $F(c)$.

I have been at the trouble of calculating the inferior limit afforded by the second approximation, and find that for b indefinitely small it is $\log \frac{2}{b} + \frac{\pi}{3\sqrt{3}}$, i. e. $\log \frac{1}{b} + 1.2977$; the superior limit has been shown to be $\log \frac{1}{b} + 1.4785$, the mean is therefore $\log \frac{1}{b} + 1.3881$, differing by only .0018 from the true value! As the constant continues for all values of i to be a multiple of π , the i th approximations *à suprà* and *ab infrà*, which are always effectible, will give (on making $i=\infty$) two new expansions for π , one infinitesimally in excess, the other infinitesimally in defect of its true value expressed as a multiple of $\log 2$, which it might well repay the trouble of some young analyst to develope.

* That the fractional forms derived from the linear substitutive form are not the best of their respective kinds, appears immediately, so far as the

maximum value of $\left(\frac{V}{U} - 1\right)^2$ for values of x intermediate between a and b shall be the least possible. Some little way, but only a little way, into the solution of this problem we can look in advance. In the first place, if we seek for the maximum values of $\left(\frac{V}{U} - 1\right)^2$, we obtain the rational equation

$$\sqrt{a+bx+cx^2} \cdot \left(U \frac{dV}{dx} - V \frac{dU}{dx}\right) = 0,$$

derivatives of the odd order (subsequent to the first) are concerned, from the consideration that the limits of error in excess and in defect will be actually attained for values of x lying within the prescribed limits; but these errors, ϵ_i and η_i (when $\epsilon = \eta$, which is true by hypothesis), are never equal, the former (the extreme error in defect) being always the greater of the two; but if any such derivative were the best of its kind, the absolute values of the extreme errors of excess and defect ought to be equal to each other. But more generally, if possible, let the i th derivative to $L(x)$ (where $L(x)$ represents the radical linear approximant $\sqrt{a+bx+cx^2}$)

to $\sqrt{a+bx+cx^2}$, say $Q(x)$), viz. $\frac{(L(x)+Q(x))^i + (LxQx)^i}{(Lx+Qx)^i - (LxQx)^i} Q(x)$, be supposed the best of its kind: then the relative error is

$$\frac{2(Lx - Qx)^i}{(Lx + Qx)^i - (Lx - Qx)^i}$$

and the maximum value of this must be equal (to the sign *près*) to the value which it has when we give to x either of its extreme connecting values.

Now obviously the above is a maximum only when $\frac{Lx + Qx}{Lx - Qx}$ is a minimum,

and therefore when $\frac{Lx}{Qx}$ is a maximum; but by hypothesis, the value of x ,

say m , which makes this a maximum, gives to $\frac{Lx}{Qx} - 1$ the same value with the opposite sign to that which it would have in writing for x either of its limiting values, say k or k' .

Thus we have two equations for determining $\frac{Lk}{Qk}$, $\frac{L(m)}{Q(m)}$, viz.

$$\frac{Lm}{Qm} - 1 = 1 - \frac{Lk}{Qk},$$

$$\text{and } \left(\frac{Lm}{Qm} + 1\right)^i - \left(\frac{Lm}{Qm} - 1\right)^i = (-)^{i-1} \left\{ \left(\frac{Lk}{Qk} + 1\right)^i - \left(\frac{Lk}{Qk} - 1\right)^i \right\}.$$

Thus, suppose $i=2$, we should obtain from the second equation $\frac{Lm}{Qm} = -\frac{Lk}{Qk}$, which is inconsistent with the first; so if $i=3$, we should

obtain $\left(\frac{Lm}{Qm}\right)^2 = \left(\frac{Lk}{Qk}\right)^2$, and therefore, on account of the first equation,

$\frac{L(m)}{Q(m)} = 1$; and so in like manner for any value of i , we should derive one

or more *numerical* values for $\frac{Lm}{Qm}$, which is absurd, since this quantity is a function of k, k' , the two connecting values of x .

which will easily be seen to be a *cubic* (not a *biquadratic*) equation in x . Call $\left(\frac{V}{U}-1\right)=\phi(x)$; then the three roots of this equation being named x_1, x_2, x_3 , the law of equality explained in my preceding paper would seem to show* that we must be able to satisfy the following equations,

$$(\phi x_1)^2 = (\phi x_2)^2 = (\phi x_3)^2 = (\phi a)^2 = (\phi b)^2,$$

which amount to four independent equations, the precise number of constants λ, μ, ν, q to be determined. So in like manner the i th rational *approximant* will contain $2i$ disposable constants; the differentiation of the quantity analogous to $\frac{V}{U}$ will give rise to an equation of the $(2i-1)$ th degree; and there will be $2i-1+2$, that is, $2i+1$ functions of these $2i$ quantities to be equated, which furnish precisely the required number of equations to make the problem definite. It is, however, apparent that in solving these equations we shall find a *multiplicity* of systems, by which I mean a *definite* number of systems of values of the disposable constants which will equally well satisfy the equations. For instance, in the theory of the second approximation, the equalities $(\phi x_1)^2 = (\phi x_2)^2 = (\phi x_3)^2$ will be satisfied by supposing $x_1 = x_2 = x_3$ †. But it is by no means evident *à priori* that this system of equalities will correspond to the absolute minimum of which we are in quest: nay, though even we had $\phi x_1 = \phi x_2 = \phi x_3$, those equations do not necessarily imply $x_1 = x_2 = x_3$. Of the multiplicity of solutions referred to, one only gives the true minimum; but to assign *à priori* the distinguishing marks of

* Is it not, however, somewhat uncertain whether the equalities

$$(\phi x_1)^2 = (\phi x_2)^2 = (\phi x_3)^2$$

must all, in all cases (that is to say, for all given values of the limits) subsist? since the law of equality will not apply to such values of x as lie without the prescribed limits, and *non constat à priori* that the roots of the cubic do all lie within these limits. The subject at the very threshold is beset with doubts and difficulties of a peculiar kind, which we can hardly hope to overcome without calling in geometrical imagination to our aid.

† If this is so, we shall have for determining the four constants the following equations:

$$x_1 = x_2 = x_3, \quad \phi a = \phi b = -\phi x_1.$$

But more probable than this seems the conjecture, that, supposing x_1, x_2, x_3 to be arranged in the order of their relative magnitudes, the determining equations might be

$$x_1 = x_3, \quad \phi a = \phi b = \phi x_2 = -\phi x_1.$$

Or is it possible that the *character* of the solution may be discontinuous, and may depend upon the magnitudes, relative or absolute, of the given limits a and b ? Probably Dr. Tchebitcheff would be able better than any other living analyst to answer these queries. But what an endless vista of future research does the prosecution of the Ponceletic method open out to us!

this truest and best, *hic labor, hoc opus est*. It will be delightful to find, if it turn out to be true, that for the best form, $\frac{P}{Q}$ representing \sqrt{x} (P being a rational function of the i th degree, and Q of the $(i-1)$ th in x), the rational quantity

$$XQ \frac{dP}{dx} - \sqrt{x} P \frac{d}{dx} (Q \sqrt{x})$$

must be a perfect $(2i-1)$ th power of a linear function of x ; but in the present state of my ignorance I dare not do more than affirm that there is a bare probability in favour of this being true: whoever shall first succeed in discovering the true form of the expression will have established a remarkable theorem. Here for the moment I break off, contented with having pointed to a theory as yet, if the expression may be allowed, sleeping in its cradle, but destined, I am persuaded, at no distant day to set in motion as large a mass of algebraical thought as has been set in motion by the never-to-be-forgotten Hessian discussion of the flexures of the cubic curve,—the turning-point between the old algebra and the new.

Henceforward Poncelet's theorem figures no longer as a detached method, a mere stroke of art in aid of the computer, but becomes integrally attached to the grand and progressive body of doctrine of the modern algebra.

XL. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 239.]

February 23, 1860.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

THE following communication was read:—

“Measurement of the Electromotive Force required to produce a Spark in Air between parallel metal plates at different distances.” By Professor W. Thomson, F.R.S.

The electrometers used in this investigation were the absolute electrometer and the portable electrometer described in my last communication to the Royal Society, and the operations were executed by the same gentlemen, Mr. Smith and Mr. Ferguson. The conductors between which the sparks passed were two unvarnished plates of a condenser, of which one was moved by a micrometer screw, giving a motion of $\frac{1}{25}$ of an inch per turn, and having its head divided into 40 equal parts of circumference. The readings on the screw-head could be readily taken to tenth parts of a division, that is to say, to $\frac{1}{10,000}$ of an inch on the distance to be measured.

The point from which the spark would pass in successive trials being somewhat variable and often near the edges of the discs, a thin flat piece of metal, made very slightly convex on its upper surface like an extremely flat watch-glass, was laid on the lower plate. It was then

found that the spark always passed between the crown of this convex piece of metal and the flat upper plate. The curvature of the former was so small, that the physical circumstances of its own electrification near its crown, the opposite electrification of the opposed flat surface in the parts near the crown of the convex, and the electric pressure on or tension in the air between them could not, it was supposed, differ sensibly from those between two plane conducting surfaces at the same distance and maintained at the same difference of potentials.

The reading of the screw-head corresponding to the position of the moveable disc when touching the metal below, was always determined electrically by making a succession of sparks pass, and approaching the moveable disc gradually by the screw until all appearance of sparks ceased. Contact was thus produced without any force of pressure between the two bodies capable of sensibly distorting their supports.

With these arrangements several series of experiments were made, in which the differences of potentials producing sparks across different thicknesses of air were measured first by the absolute electrometer, and afterwards by the portable torsion electrometer. The following Tables exhibit the results hitherto obtained.

TABLE I.—December 13, 1859. Measurements by absolute electrometer of maximum electrostatic forces* across a stratum of air of different thicknesses.

Area of each plate of absolute electrometer = $\cdot 187$ of a square foot.

Distance between plates of absolute electrometer = $\frac{1}{60}$ of a foot.

Length of spark in inches. <i>s.</i>	Weight in grains required to balance in absolute electrometer. <i>w.</i>	Electromotive force in units of the electrometer. $\sqrt{w}.$	Electrostatic force, or electromotive force per inch of air, in temporary units. $\frac{\sqrt{w}}{s}.$
$\cdot 007$	6	2.4495	349.9
$\cdot 0105$	9	3.0000	285.7
$\cdot 0115$	10	3.1622	275.0
$\cdot 014$	13	3.6055	257.5
$\cdot 017$	16	4.0000	235.3
$\cdot 018$	19	4.3589	242.2
$\cdot 024$	30	5.4772	228.2
$\cdot 0295$	40	6.3245	214.4
$\cdot 034$	50	7.0710	208.0
$\cdot 0385$	60	7.7459	201.2
$\cdot 041$	70	8.3666	204.1
$\cdot 0445$	80	8.9442	201.0
$\cdot 048$	90	9.4868	197.6
$\cdot 052$	100	10.0000	192.3
$\cdot 055$	110	10.4880	190.7
$\cdot 058$	120	10.9544	188.9
$\cdot 060$	130	11.4017	190.0

* See § 3 below.

These numbers demonstrate an unexpected and a very remarkable result,—that greater electromotive force per unit length of air is required to produce a spark at short distances than at long. When it is considered that the absolute electrification of each of the opposed surfaces* depends simply on the electromotive force per unit length of the space between them, or, which is the same thing, the resultant electrostatic force in the air occupying that space, it is difficult even to conjecture an explanation. Without attempting to explain it, we are forced to recognize the fact that a thin stratum of air is stronger than a thick one against the same disruptive tension in the air, according to Faraday's view of its condition as transmitting electric force, or against the same lifting electric pressure from its bounding surfaces, according to the views of the 18th century school, as represented by Poisson. The same conclusion is established by a series of experiments with the previously-described portable torsion electrometer substituted for the absolute electrometer, leading to results shown in the following Table.

TABLE II.—January 17, 1860. Measurements by portable torsion electrometer of electromotive forces producing sparks across a stratum of air of different thicknesses.

Length of spark in inches. s.	Torsion in degrees required to balance in electrometer. θ .	Electromotive force in units of the electrometer. $\sqrt{\theta}$.	Electrostatic force, or electromotive force per inch of air, in tem- porary units. $\sqrt{\theta \div s}$.
·001	3	1·732	1732
·002	7	2·646	1323
·003	11	3·316	1105
·004	14	3·742	935
·005	18	4·243	849
·006	22	4·690	782
·007	27	5·196	742
·008	30	5·477	685
·009	33	5·744	638
·010	38	6·164	616
·011	43	6·557	596
·012	48·5	6·964	580
·013	54	7·348	565
·014	59	7·681	549
·015	66	8·124	542
·016	73	8·544	534
·017	79	8·888	523
·018	85	9·219	512

The series of experiments here tabulated stops at the distance 18 thousandths of an inch, because it was found that the force in the

* See § 4 below.

electrometer corresponding to longer sparks than that, was too strong to be measured with certainty by the portable electrometer, whether from the elasticity of the platinum wire, or from the rigidity of its connexion with the aluminium index being liable to fail when more than 85° or 90° of torsion were applied. So far as it goes, it agrees remarkably well with the other experiments exhibited in Table I., as is shown by the following comparative Table, in which, along with results of actual observation extracted from Table II., are placed results deduced from Table I. by interpolation for the same lengths of spark.

TABLE III. Experiments of December 13, 1859, and January 17, 1860, compared.

Col. 1. Length of spark in inches. s.	Col. 2. Electromotive force per inch of air, Dec. 13, in tempo- rary units of that day. $\frac{\sqrt{w}}{s}$	Col. 3. Electromotive force per inch of air, Jan. 17, in tem- porary units of that day. $\frac{\sqrt{\theta}}{s}$	Col. 4. Ratios of numbers in Col. 3. to numbers in Col. 2.
·007	349·3	742	2·13
·0105	285·7	606	2·12
·0115	275·0	588	2·14
·014	257·5	549	2·14
·017	235·3	523	2·22
·018	242·2	512	2·11
			Mean 2·14

The close agreement with one another of the numbers in Col. 4, derived from series differing so much as those in Cols. 2 and 3, and obtained by means of electrometers differing so much in construction, constitutes a very thorough confirmation of the remarkable result inferred above from the experiments of the first series, and shows that the law of variation of the electrostatic force in the air required to produce sparks of the different lengths, must be represented with some degree of accuracy by the numbers shown in the last column of either Table I. or Table III.

The following additional series of experiments were made on precisely the same plan as those of Table II.

TABLE IV.—January 21, 1860. Measurements by portable torsion electrometer of electromotive forces producing sparks across a stratum of air of different thicknesses.

Length of spark in inches. <i>s.</i>	Torsion in degrees required to balance in electrometer. θ .	Electromotive force in units of the electrometer. $\sqrt{\theta}$.	Electrostatic force, or electromotive force per inch of air, in tem- porary units. $\sqrt{\theta} \div s$.
·001	3·2	1·79	1790
·002	6·4	2·32	1160
·003	10·5	3·24	1080
·004	13·2	3·63	907
·005	14·2	3·77	754
·006	18·2	4·27	712
·007	21·7	4·66	666
·012	41·2	6·42	535
·013	46·7	6·83	525
·014	53·2	7·29	521
·015	57·2	7·56	504
·016	63·2	7·95	497
·017	68·2	8·26	486
·018	78·2	8·84	491

TABLE V.—January 23, 1860. Similar experiments repeated.

<i>s.</i>	θ .	$\sqrt{\theta}$.	$\sqrt{\theta} \div s$.
·001	3·5	1·87	1870
·002	6·5	2·55	1275
·003	9·5	3·08	1027
·004	12·7	3·56	890
·005	15·5	3·94	788
·006	18·5	4·30	716
·007	23·0	4·80	686
·008	25·62	5·06	632
·009	30·5	5·52	613
·010	35·0	5·92	592
·011	39·5	6·28	571
·012	44·0	6·63	553
·013	50·0	7·07	544
·014	54·0	7·35	525
·015	59·0	7·68	512
·016	63·5	7·97	498
·017	69·5	8·34	490
·018	74·5	8·63	479

The difference between the numbers shown in these two Tables and in Table II. above, are probably due in part to true differences in the resistance of the air to electrical disruption; but variations in the electrometer, which was by no means of perfect construction, may have sensibly influenced the results, especially as regards the

differences between those shown in Table II. and those shown in Tables IV. and V., which agreeing on the whole closely with one another fall considerably short of the former.

TABLE VI. Summary of results reduced to absolute measure.

Col. 1. Length of spark in inches. s.	Col. 2. Electrostatic forces according to simple determinations of Dec. 13, 1859. $\frac{\sqrt{w}}{s} \times \frac{1}{5} \times \sqrt{\frac{32.2 \times 8\pi^*}{.187}}$ =X.	Col. 3. Electrostatic forces according to estimated average of various determinations. X.	Col. 4. Differences.	Col. 5. Pressures of electricity from either metallic surface balanced by air immediately before disruption, in grains weight per square foot †. $\frac{X^2}{8\pi \times 32.2}$
.001	...	11480	...	162800
.002	...	8000	...	79080
.003	...	6840	...	57810
.004	...	5820	...	41820
.005	...	5090	...	32010
.006	...	4700	...	27300
.007	4600	4530	+ .0070	26200
.008	...	4200	...	21830
.009	...	3990	...	19690
.010	...	3860	...	18390
.0105	3760	3770	- .0010	17450
.011	...	3720	...	17130
.0115	3620	3630	- .0010	16200
.012	...	3550	...	15580
.013	...	3480	...	14970
.014	3390	3390	.0000	14200
.015	...	3320	...	13580
.016	...	3260	...	13110
.017	3000	3140	- .0040	11800
.018	3190	3170	+ .0020	12500
.024	3100	3000	...	11100
.0295	2820	2820	...	9830
.034	2740	2740	...	9250
.0385	2650	2650	...	8650
.041	2690	2690	...	8900
.0445	2650	2650	...	8640
.048	2600	2600	...	8350
.052	2530	2530	...	7910
.055	2510	2510	...	7770
.058	2490	2490	...	7630
.060	2500	2500	...	7720

* Distance between discs of absolute electrometer = $\frac{1}{80}$ foot = $\frac{1}{5}$ inch.

Area of each = .187 square foot.

Force of gravity at Glasgow on unit mass = 32.2 dynamical units of force; that is to say, generates in one second a velocity of 32.2 feet per second.

† This is most directly obtained by finding the force between the discs of the absolute electrometer per square foot, and reducing, according to the inverse proportion of squares of distances, to what it would have been if the distance between them had been equal to the length of the spark.

APPENDIX.

In order that the different expressions, "potential," "electromotive force," "electrostatic force," "pressure of electricity from a metallic surface balanced by air," used in the preceding statement, may be perfectly understood, I add the following explanations and definitions belonging to the ordinary elements of the mathematical theory of electricity.

1. *Measurement of quantities of electricity.*—The unit quantity of electricity is such a quantity, that, if collected in a point, it will repel an equal quantity collected in a point at a unit distance with a force equal to unity.

[In British absolute measurements the unit distance is one foot ; and the unit force is that force which, acting on a grain of matter during a second of time, generates a velocity of one foot per second. The weight of a grain at Glasgow is 32·2 of these British units of force. The weight of a grain in any part of the earth's surface may be estimated with about as much accuracy as it can be without a special experiment to determine it for the particular locality, by the following expression :—

In latitude λ average weight of a grain

$$= 32\cdot088 \times (1 + \cdot005133 \times \sin^2 \lambda) \text{ British absolute units.}]$$

2. *Electric density.*—This term was introduced by Coulomb to designate the quantity of electricity per unit of area in any part of the surface of a conductor. He showed how to measure it, though not in absolute measure, by his proof plane.

3. *Resultant electric force at any point in an insulating fluid.*—The resultant force at any point in air or other insulating fluid in the neighbourhood of an electrified body, is the force which a unit of electricity concentrated at that point would experience if it exercised no influence on the electric distributions in its neighbourhood.

4. *Relation between electric density on the surface of a conductor, and electric force at points in the air close to it.*—According to a proposition of Coulomb's, requiring, however, correction, and first correctly given by Laplace, the resultant force at any point in the air close to the surface of a conductor is perpendicular to the surface and equal to $4\pi\rho$, if ρ denotes the electric density of the surface in the neighbourhood.

5. *Electric pressure from the surface of a conductor balanced by air.*—A thin metallic shell or liquid film, as for instance a soap-bubble, if electrified, experiences a real mechanical force in a direction perpendicular to the surface outwards, equal in amount per unit of area to $2\pi\rho^2$, ρ denoting, as before, the electric density at the part of the surface considered. This force may be called either a repulsion (as according to the views of the eighteenth century school) or an attraction effected by tension of air between the surface of the conductor and the conducting boundary of the air in which it is insulated, as it would probably be considered to be by Faraday ; but whatever may be the explanation of the *modus operandi* by which it is produced, it

is a real mechanical force, and may be reckoned as in Col. 5 of the preceding Table, in grains weight per square inch or per square foot. In the case of the soap-bubble, for instance, its effect will be to cause a slight enlargement of the bubble on electrification with either vitreous or resinous electricity, and a corresponding collapse on being perfectly discharged. In every case we may regard it as constituting a deduction from the amount of air-pressure which the body experiences when unelectrified. The amount of this deduction being different in different parts according to the square of the electric density, its resultant action on the whole body disturbs its equilibrium, and constitutes in fact the resultant electric force experienced by the body.

6. *Collected formulæ of relation between electric density on the surface of a conductor, electric diminution of air-pressure upon it, and resultant force in the air close to the surface.*—Let, as before, ρ denote the first of these three elements, let p denote the second reckoned in units of force per unit of area, and let R denote the third. Then we have

$$R = 4\pi\rho,$$

$$p = 2\pi\rho^2 = \frac{1}{8\pi} R^2.$$

7. *Electric potential.*—The amount of work required to move a unit of electricity against electric repulsion from any one position to any other position, is equal to the excess of the electric potential of the first position above the electric potential of the second position.

Cor. 1. The electric potential at all points close to the surface of an electrified metallic body has one value, since an electrified point, possessing so small a quantity of electricity as not sensibly to influence the electrification of the metallic surface, would, if held near the surface in any locality, experience a force perpendicular to the surface in its neighbourhood.

Cor. 2. The electric potential throughout the interior of a hollow metallic body, electrified in any way by external influence, or, if insulated, electrified either by influence or by communication of electricity to it, is constant, since there is no electric force in the interior in such circumstances.

[It is easily shown by mathematical investigation, that the electric force experienced by an electric point containing an infinitely small quantity of electricity, when placed anywhere in the neighbourhood of a hollow electrified metallic shell, gradually diminishes to nothing if the electric point be moved gradually from the exterior through a small aperture in the shell into the interior. Hence the one value of the potential close to the surface outside, mentioned in *Cor. 1*, is equal to the constant value throughout the interior mentioned in *Cor. 2*.]

8. *Interpretation of measurement by electrometer.*—Every kind of electrometer consists of a cage or case containing a moveable and a fixed conductor, of which one at least is insulated and put in metallic communication, by what I shall call the principal electrode passing

through an aperture in the case or cage, with the conductor whose electricity is to be tested. In every properly constructed electrometer, the electric force experienced by the moveable part in a given position cannot be electrically influenced except by changing the difference of potentials between the principal electrode and the uninsulated conductor or conducting system in the electrometer. Even the best of ordinary electrometers hitherto constructed do not fulfil this condition, as the inner surface of the glass of which the whole or part of the enclosing case is generally made, is liable to become electrified, and inevitably does become so when any very high electrification is designedly or accidentally introduced, even for a very short time; the consequence of which is that the moving body will generally not return to its zero position when the principal electrode is perfectly disinsulated. Faraday long ago showed how to obviate this radical defect by coating the interior of the glass case with a fine network of tinfoil; and it seems strange that even at the present day electrometers for scientific research, as for instance for the investigation of atmospheric electricity, should be constructed with so bad and obvious a defect uncured by so simple and perfect a remedy. When it is desired to leave the interior of the electrometer as much light as possible, and to allow it to be clearly seen from any external position with as little embarrassment as possible, a cage made like a bird's cage, with an extremely fine wire on a metal frame, inside the glass shade used to protect the instrument from currents of air, &c., may be substituted with advantage for the tinfoil network lining of the glass. It appears therefore that a properly constructed electrometer is an instrument for measuring, by means of the motions of a moveable conductor, the difference of potentials of two conducting systems insulated from one another, of one of which the case or cage of the apparatus forms part. It may be remarked in passing, that it is sometimes convenient in special researches to insulate the case or cage of the apparatus, and allow it to acquire a potential differing from that of the earth, and that then, as always, the subject of measurement is the difference of potentials between the principal electrode and the case or cage, while in the ordinary use of the instrument the potential of the latter is the same as that of the earth. Hence we may regard the electrometer merely as an instrument for measuring differences of potential between two conducting systems mutually insulated; and the object to be aimed at in perfecting any kind of electrometer (more or less sensitive as it may be, according to the subjects of investigation for which it is to be used), is, *that accurate evaluations in absolute measure, of differences of potential, may be immediately derivable from its indications.*

9. *Relation between electrostatic force and variation of electric potential.*—§ 7, otherwise stated, is equivalent to this:—The average component electrostatic force in the straight line of air between two points in the neighbourhood of an electrified body is equal to their difference of potentials divided by their distance. In other words, the rate of variation of electric potential per unit of length in any direction, is equal to the component of the electrostatic force in

that direction. Since the average electrostatic force in the line joining two points at which the values of the potential are equal, is nothing, the direction of the resultant electrostatic force at any point must be perpendicular to the equipotential surface passing through that point; or the lines of force (which are generally curves) cut the series of equipotential surfaces at right angles. The rate of variation of potential per unit of length along a line of force is therefore equal to the electrostatic force at any point.

10. *Stratum of air between two parallel or nearly parallel plane or curved metallic surfaces maintained at different potentials.*—Let a denote the distance between the metallic surfaces on each side of the stratum of air at any part, and V the difference of potentials. It is easily shown that the resultant electrostatic force is sensibly constant through the whole distance, from the one surface to the other; and being in a direction sensibly perpendicular to each, it must (§ 9)

be equal to $\frac{V}{a}$. Hence (§ 4) the electric density on each of the

opposed surfaces is equal to $\frac{V}{4\pi a}$. This is Green's theory of the Leyden phial.

11. *Absolute Electrometer.*—As a particular case of No. 10, let the discs be plane and parallel; and let the distance between them be small in comparison with their diameters, or with the distance of any part of either from any conductor differing from it in potential. The electric density will be uniform over the whole of each of the

opposed surfaces and equal to $\frac{V}{4\pi a}$, being positive on one and negative on the other; and in all other parts of the surface of each the electrification will be comparatively insensible. Hence the force of

attraction between them per unit of area (§§ 5 and 6) will be $\frac{V^2}{8\pi a^2}$,

if A denote the area of either of the opposed surfaces; the whole

force of attraction between them is therefore $A \frac{V^2}{8\pi a^2}$. Hence, if the

observed force be equal to the weight of w grains at Glasgow, we have

$$32 \cdot 2 \times w = A \frac{V^2}{8\pi a^2},$$

and therefore

$$V = a \sqrt{\frac{32 \cdot 2 \times 8\pi \times w}{A}}.$$

Addition, dated April 12, 1860.

Experiments on precisely the same plan as those of Table I. December 13, have been repeated by the same two experimenters, with different distances from $\cdot 3$ to $\cdot 6$ of an inch between the plates

of the absolute electrometer, and results have been obtained confirming the general character of those shown in the preceding Tables.

The absolute evaluations derived from these later series, must be more accurate than those deduced above from the single series of December 13, when the distance between the plates in the absolute electrometer was only $\cdot 2$ of an inch. I therefore by permission add the following Table of absolute determinations :—

Length of spark in inches. <i>s.</i>	Electrostatic forces according to estimated average of determina- tions of February 15, 23, 28, and 29, and March 2. X.
$\cdot 0034$	5793
$\cdot 005$	5574
$\cdot 006$	5688
$\cdot 0075$	4862
$\cdot 0111$	4351
$\cdot 0161$	3287
$\cdot 0222$	3125
$\cdot 023$	3029
$\cdot 0271$	3055
$\cdot 0356$	2925
$\cdot 0416$	2865
$\cdot 0522$	2841

These results, as well as those shown in the preceding Tables, demonstrate a much less rapid variation with distance, of the electrostatic force preceding a spark, at the greater than at the smaller distances. It seems most probable that at still greater distances the electrostatic force will be found to be sensibly constant, as it was certainly expected to be at all distances. The limiting value to which the results shown in the last Table seem to point must be something not much less than 2800. This corresponds to a pressure of 9600 grains weight per square foot. We may therefore conclude that the ordinary atmospheric pressure of 14,798,000 grains per square foot, is electrically relieved by the subtraction of 9600 on two very slightly convex metallic surfaces, at a distance of $\frac{1}{20}$ th of an inch or more, before the air between them is cracked and a spark passes. By taking into account the result of my preceding communication to the Royal Society, we may also conclude that a Daniell's battery of 5510 elements can produce a spark between two slightly convex metallic surfaces at $\frac{1}{20}$ th of an inch asunder in ordinary atmospheric air.

XII. *Intelligence and Miscellaneous Articles.*

ON THE DIFFERENCE IN SIZE OF MEDALS OF DIFFERENT METALS OBTAINED BY STAMPING, AND BY CASTING IN THE SAME MOULD. BY H. W. DOVE.

BAUDRIMONT has found (*Ann. de Chim. et de Phys.* vol. lx. p. 78) that wires of different metals drawn through the same press are not all of the same thickness; for they are of different degrees of elasticity, and after being drawn through the press they expand to different amounts. This expansion is proved by the fact that, with the exception of gold wire, no wire can be drawn through the same aperture through which it has been pressed. Silver requires the least force, but the expansion caused by elasticity continues for several weeks.

It appeared probable that in stamping medals something similar would prevail, and that medals of different metals stamped in the same die would be different in size. This is most readily seen in those medals in which the impression is symmetrically arranged in reference to the edge, as is the case with the medals of the French Exhibition, in which the coats of arms encircle the French eagle in the middle. One of those in silver, and one in bronze were placed in the stereoscope, the eagle being fixed in the middle. After some time the stereoscopic combined medal was seen in the form of a hollow escutcheon, and of the colour of an alloy of the two metals. Evidently the reason of this lies in the nonius-like shifting of the individual lines of the impression. This result, which I have described (*Optische Studien*, p. 29), I have also obtained with large gold and silver medals which were kindly entrusted to me from the Royal Mint in Berlin. It was probable that medals obtained by casting would show the same thing, and this was found to be the case with tin, bismuth, and lead. The casts were very beautifully executed for me for this purpose by Professor Kiss. Hiero's crown led to the application of specific gravity to detect an adulteration; the stereoscope is a new means.—Poggendorff's *Annalen*, vol. cx. p. 498.

ON ATMOSPHERIC ELECTRICITY. BY M. VOLPICELLI.

Of the two characters which distinguish electricity, its tension and its nature, the first depends on the hygrometric state of the atmosphere, and the second is independent of it. The author describes a series of experiments on the latter point made on days which were not stormy.

1st. The upper extremity of a copper rod, well insulated and fixed on the roof of the Physical Museum of the University of Rome, was 45^m·39 high above the sea-level. If the upper end terminated

in a point or a ball, and the lower end in a gold-leaf electroscope, it seldom showed the existence of atmospheric electricity. But when the lower end was connected with a dry pile *condensing* electroscope, signs of electricity were always obtained, sometimes positive and sometimes negative; and this is the only means by which, with a fixed rod, the nature of the atmospheric electricity may *always* be obtained. Before commencing the experiment, it must be ascertained if the instrument is in the natural state; this is the case when, after touching both plates and then separating them, the gold leaf remains at rest. In the second place, the electricity must be collected first by the upper, and then by the lower plate, in order to ascertain that the two results agree. After each experiment both plates of the condenser must be connected with the ground, and separated from one another by an uninsulated metallic foil. These precautions are essential in detecting the small traces of electricity usually met with in the atmosphere.

2nd. The nature of the atmospheric electricity sometimes varies four or five times in the space of three or four minutes.

3rd. The electricity is the same, whether the rod terminates in a point or in a ball; the quantity varies little, but seems less with a ball than a point.

4th. When a flame, or an incandescent ball, or even ignited charcoal is placed on the point, the electricity which was *negative* is changed into *positive*. If the atmospheric electricity is positive, flame and incandescent metals simply increase the intensity. The hotter the flame, the greater the quantity of electricity. The effects of a spirit-lamp exceed those of an oil-lamp. Hence flame is liable to introduce a source of error, when used in making observations on atmospheric electricity.

5th. If, in some very rare cases, flame does not change the negative electricity exhibited by the point into positive electricity, it not only does not increase the tension, but seems to diminish it, and thus another source of serious perturbations is introduced into the observations.

6th. In a room and by the methods described, the author has always obtained traces of positive electricity by means of flame.—*Comptes Rendus*, July 16, 1860.

MAGNETIC PHENOMENON.

M. Ruhmkorff has the following notice in the *Comptes Rendus*, vol. 1. p. 166:—"If a stay (*bride*) of soft iron be pressed against one of the poles of an artificial magnet, the soft iron is observed to become hard, it is more difficult to file. If the stay be removed, it loses its hardness and resumes all the properties of soft iron."

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XLII. *On a new Species of Stereoscopic Phænomenon.*
By F. AUGUST*.

[With a Plate.]

THE object of this paper is to communicate an experiment which seems to have an important bearing on the theory of binocular vision.

Wheatstone, it will be remembered†, has called in question the doctrine of identical retina-points, on certain grounds connected with stereoscopic vision. A body of suitable dimensions, especially at the distance of distinct vision, will, he says, appear simply solid, though it is obviously impossible that the corresponding points of the images received by the two eyes can fall on the corresponding points of the retinae. This objection, however, has not prevailed on physiologists to abandon the doctrine of corresponding retina-points, since that doctrine is confirmed by many other considerations. Brücke, indeed, has advanced‡ an explanation of stereoscopic vision which has hitherto been generally regarded as sufficient, and which at first sight appears to afford a solution of the difficulty. According to Brücke, the eyes during vision are never at rest, but are constantly making small movements, by means of which the images of continually different points of the body regarded fall successively in the two eyes on corresponding points of the retinae; and thus single vision arises, since the images of points which fall on parts of the eyes that do not correspond escape consciousness, because in general the recognition of double impressions is resisted. The impressions, however, perceived by corresponding retina-points

* Translated from Poggendorff's *Annalen* for 1860, by F. Guthrie, Esq.

† Phil. Trans. 1838, vol. ii. p. 371. Pogg. Ann. Suppl. vol. i. p. 1.

‡ Müller's *Archiv*, 1841.

survive the period of the eyes' vibration (if the expression may be allowed); and thus we have the durable representation of the entire body, whose dimensions we estimate by the difference of the convergence of corresponding rays. To this theory Dove has objected that the two images of a stereoscope may be seen united so as to give the impression of solidity by the almost instantaneous illumination of the electric spark: this, however, may be explained on the grounds of an incredibly rapid movement of the eye, however improbable that may be. Brücke's theory has therefore continued to be generally received without any essential modification, even on account of Meissner's* profound research concerning the horopter and the position of the corresponding retina-points, or of the controversy connected therewith.

2. The experiment above-mentioned is as follows:—A thin cylinder of metal, as straight and smoothly polished as possible, is so united with a fixed axis that its own geometrical axis cuts the other at right angles. The experiment may be performed with sufficient accuracy with two needles fixed in a cork at right angles to each other. The one needle forms the rotating cylinder, the other the axis of rotation. If the cylinder be placed at rest in the sunlight and regarded on either side with one eye, then, provided its length be sufficient, the reflexion of the sun, or rather a bright spot (which we shall call the point of reflexion), will be observed somewhere in its length. If now the first eye be shut and the cylinder be regarded with the other, the point of reflexion will be perceived in another position. On opening both eyes, two points will be seen at the same time in different places; and if lines be imagined drawn from each of these points to the corresponding eye (which lines may be called the reflected rays "corresponding to each eye"), it is obvious that these lines do not lie in the same plane except when the cylinder itself lies in the same plane with the line joining the optic centres of both eyes, which we shall call as usual the "ground-line." If the cylinder be rotated, each of the points of reflexion will obviously change its place on the cylinder, and will therefore describe a curve in the plane of rotation; and this curve, if the rotation be sufficiently rapid, will appear continuously illuminated. A somewhat different curve will of course appear when the apparatus is regarded with the other eye. If, however, the apparatus be regarded with both eyes, instead of two curves in the plane of rotation as might be expected, a single curve of double curvature is generally perceived, not confined to the plane of rotation, but formed by the intersection of the conical surfaces described by the two reflected rays during rotation.

* *Beiträge zur Physiologie des Sehorgans.* Leipzig, 1854.

3. Before the further consideration of this experiment and its consequences, the following short analytical introduction may be permitted.

Let the axis of rotation be the axis of z , the plane in which the cylinder rotates be the plane xy . Let the eye A be at the distance r from the origin O; the line OA making the angles α, β, γ with the axes of x, y , and z respectively. And let the angles made by the parallel incident rays with the same axes be called α_1, β_1 , and γ_1 . The axes of x and y may, however, be so taken that the axis of y is in the plane passing through the axis of z and the ray of light incident on O, so that $\alpha_1 = 90^\circ, \beta_1 = 90^\circ - \gamma_1$. Let the angle formed at any moment by the cylinder (which must be regarded as infinitely thin) with the axis of x be called ϕ . We have now to inquire what is the distance of the point of reflexion R from the origin O. Let the distance in question be called ρ . In the first place, the angle ROA is easily determined as follows:

$$\cos ROA = \cos \phi \cos \alpha + \sin \phi \cos \beta.$$

The angle ARO made by the reflected ray with the cylinder must, from the property of the cylindrical surface, be equal to that made by the incident ray, whence

$$\cos ARO = \sin \gamma_1 \sin \phi.$$

In the triangle AOR we know, therefore, the angles ROA and ARO, and the side AO = r . Whence we get

$$AR = \rho = \frac{r \sin (ARO + AOR)}{\sin (ARO)};$$

which, on substituting the values of ARO and AOR, becomes

$$\rho = r \left[\cos \phi \cos \alpha + \sin \phi \cos \beta + \sin \gamma_1 \sin \phi \sqrt{\frac{1 - (\cos \phi \cos \alpha + \sin \phi \cos \beta)^2}{1 - \sin^2 \gamma_1 \sin^2 \phi}} \right],$$

a curve of the sixth order, closed, unless $\gamma_1 = 90^\circ$, and passing through the origin.

When the incident rays are parallel to the axis of rotation, $\gamma_1 = 0$, and the part of the above expression containing the square root disappears. The equation being then referred to rectangular coordinates, becomes

$$\left(x - \frac{r}{2} \cos \alpha\right)^2 + \left(y - \frac{r}{2} \cos \beta\right)^2 = \frac{r^2}{4} \sin^2 \gamma,$$

that is to say, the equation of a circle having the projection of OA for a diameter, and being the intersection of the plane xy

and the sphere of which OA is the diameter. (This result should be observed, since it can easily be deduced that the reflected ray forms a right angle with the cylinder, according to the well-known property of the sphere.)

The equation to the conical surface having this circle as a base, and the point A as an apex, may be easily shown to be as follows :

$$(x^2 + y^2) \cos^2 \gamma - zx \cos \alpha \cos \beta - zy \cos \beta \cos \gamma + zr \sin^2 \gamma \cos \gamma \\ - rx \cos^2 \gamma \cos \alpha - ry \cos^2 \gamma \cos \beta = 0.$$

If, now, the eyes be placed symmetrically with respect to the plane yz , so that both the lines drawn from the origin to the eyes make the angle β with the axis of y , and γ with the axis of z , and α and $180^\circ - \alpha$ respectively with the axis of x , then the equation of the corresponding conical surface for the other eye will be

$$(x^2 + y^2) \cos^2 \gamma + zx \cos \alpha \cos \beta - zy \cos \beta \cos \gamma + zr \sin^2 \gamma \cos \gamma \\ + rx \cos^2 \gamma \cos \alpha - ry \cos^2 \gamma \cos \beta = 0.$$

The subtraction of the two equations gives

$$2zx \cos \alpha \cos \gamma + 2rx \cos^2 \gamma \cos \alpha = 0,$$

or

$$2 \cos \alpha \cos \gamma (z - r \cos \gamma) = 0.$$

The conical surfaces corresponding to the two eyes cut each other, therefore, in two planes, (1) $x=0$, and (2) $z = -r \cos \gamma$.

The curve in the first plane is an ellipse, hyperbola, or parabola. (The case in which it becomes a circle cannot arise.)

The curve in the second plane, $z = -r \cos \gamma$, is a circle, of which the equation referred to rectangular coordinates is

$$x^2 + y^2 = r^2 \sin^2 \gamma.$$

The particular case above considered may be very easily exhibited experimentally; the axis of rotation of the apparatus is placed parallel to the incident rays, the eyes are put in such a position that they are both equally distant from O the origin and Oz the axis of rotation. (Since $\gamma_1 = 0$, the axis of y may be taken anywhere in the axis of rotation without affecting the formulæ.) In this position of the eyes a stereoscopic image of a circle will be observed as far behind the plane of rotation as the eyes are before it, or conversely, as far before it as the eyes are behind, having its middle point in the axis of rotation, and a radius equal to the distance of either eye from the axis. It appears, therefore, that it is the second only of the two curves mentioned above that presents itself. Why this is the case shall be explained hereafter; our immediate purpose was only to verify our calculations by comparison with a simple experiment.

4. With reference to the experiment itself, it should be mentioned that the stereoscopic curve displays itself with particular distinctness in comparison with the single images when the plane of rotation is parallel to the incident rays and consequently the axis of rotation is perpendicular, and when the apparatus is regarded from behind and on one side. The single curve is, however, still more clearly discernible when there are two different sources of light; these give rise to distinct curves, from the comparison of which the stereoscopic effect is increased.

It hardly needs to be observed that, instead of the parallel rays of the sun, any other source of light with diverging rays may be employed, and that the above calculations, when the source of light is not too near, are still sufficiently accurate. Moreover the equation of the curve described by the point of reflexion when the light proceeds from a point L at the distance r_1 from O, and OL makes with the axes of x , y , and z the angles α_1 , β_1 , γ_1 , may easily be shown to be as follows:

$$\frac{r_1 \sin \text{LOR}}{\sqrt{r_1^2 + \rho^2 + 2r_1\rho \cos \text{LOR}}} = \frac{r \sin \text{AOR}}{\sqrt{r^2 + \rho^2 + 2r\rho \cos \text{AOR}}};$$

where for LOR and AOR the values found above must be substituted.

5. Though it is stated above that the two images generally unite into one, it should be observed that this property depends on the extent to which it is possible to bring them into superposition. Images which in size or form are too different cannot be stereoscopically united. As, according to the formula, ρ is directly proportional to r (the distance of the eye from the origin), stereoscopic images will be more easily perceived in proportion as the distances of the two eyes from the origin are equal; but even when these distances are the same, the angles may be so different that the images cannot, or can only with great difficulty, be superimposed.

6. The conclusion that may be drawn with certainty from the experiment is as follows. Since two points of the separate curves which unite to form one point of the stereoscopic image *are not seen at the same moment* with both eyes, it is impossible for the eyes to be so accommodated that corresponding points throw their images on corresponding points of the retinae of the two eyes. That the eyes arrange themselves by anticipation is not credible; and even if they did, for every revolution of the cylinder only one point would appear single, the rest double.

It is moreover impossible that the two curves that exist in the two eyes can be compared as wholes, and received point by point on corresponding retina-points; for since the images are only virtual, that is, only produced by the endurance of the impres-

sion in the eye, the image in an eye in motion must appear different from that in an eye at rest, while on the other hand the image of a real object must be the same in an eye in rapid motion as in one at rest. As now the virtual image of the point of reflexion moving along the curve is precisely identical as to form with the curve itself, it is impossible that the eye can make any sensible movements. Or to express the same thing in other words, no movement of the eyes would bring two virtual images on to corresponding retina-points, which do not already lie on such parts, since their images are necessarily confined to those points of the retina on which they are excited. The experiment therefore compels us to regard Brücke's theory of binocular vision as untenable, especially since it has already been rendered doubtful by Dove's objection.

7. It may be asked how the experiment can be reconciled with the well-supported doctrine of identical retina-points. With reference to this point the observations made in paragraph 5 may be of some assistance. If the curves are too dissimilar they afford no stereoscopic image; that is to say, if the images fall on parts of the retinae too remote, our imagination is unable to unite them. This is precisely the same case as when we regard an object whose dimensions are too great in proportion to its distance from the eyes; in that case only a small part of it is seen stereoscopically, all the rest appears double. The identical retina-points would thus, practically, in regard to solid vision, only have the negative signification that two particular impressions can be united into one when that in the one eye is not too far removed from the place that corresponds to the position of the impression in the other eye. If this condition be fulfilled, the solid image appears at the intersection of the two rays, which may be considered as drawn through the middle point of each eye and the points of the retinae affected; if these do not cut (if they are not in the same plane), it is impossible, as is well known, to perceive a single image. There are therefore two conditions necessary for the formation of a single solid image.

The limits within which the point of the retina impressed may vary from the true corresponding point is probably partly dependent on the will, but may be extended or reduced by practice. This at least seems to be the explanation of the fact that those accustomed to the clear observation of a point see more easily all surrounding objects double, while those who accustom themselves to stereoscopic vision can unite very remote images into one. The principal difficulty in the way of the experimental determination of these relations would arise from subjective differences.

How far this view is admissible others must decide. It is

substantially consistent with an observation made by Wheatstone on the horopter, towards the conclusion of his memoir above-mentioned, and also with the views expressed by Johannes Müller and Meissner on stereoscopic vision.

8. We can now explain why, in the experiment mentioned towards the conclusion of the third paragraph, we saw only the circle and not the conic section in the plane perpendicular to the ground line, and passing through the axis of z ($x=0$). Imagine (Pl. IV. fig. 6) that with the eye A we only saw the rays a and α ; with the eye B the rays b and β (so that the points in order are $ab, \alpha\beta$); then, on looking with both eyes, should we perceive stereoscopically the points ab and $\alpha\beta$, or the points $a\beta$ and $b\alpha$? It is clear that if the point $b\alpha$ were seen, the rays a and β would impinge on very different points of the retina (according to either Meissner's or Recklinghausen's theory), so that they would not be stereoscopically united,—so much so, indeed, that if there actually were points at $a\beta$ and $b\alpha$, they could not be seen stereoscopically at the same time. If, however, the point ab be observed, the rays α and β fall on parts of the retina nearly corresponding, and their impressions can therefore be easily united. This is the reason why the two latter points are seen and not the former. It will also be easily understood that it is precisely for the same reason that, in the above experiment, the circle was perceived and not the curve in the plane yz , because, when one point of the circle is seen stereoscopically, the images of the other corresponding points are much nearer the identical retina-points in the two eyes than is the case when one point in the curve in yz is so observed; so that if the curve in yz actually existed, it could not for this very reason be seen stereoscopically, but would always present a double image.

The foregoing observations will perhaps be rendered clearer by the particular case represented in fig. 7. Here the incident rays are supposed parallel to the axis of rotation z , which bisects the ground-line AB at right angles, the plane of rotation being the plane xy . The eye A sees the circle EGO (drawn in perspective in the figure), the eye B the circle LHO. These circles meet in O, and have a common tangent. The two cones generated by the reflected rays intersect, and are, from their position, symmetrical, the lines AE and BE' being perpendicular to the plane yz . The intersections of the two cones are (1) the parabola OKN in the plane perpendicular to AB, and (2) the circle CDC' in the plane parallel to the plane of rotation (xy). If, now, the point O, for instance, in the parabola be seen stereoscopically, in order to observe the point K, the rays AG and BL must be united, which obviously fall on very different points

of the retinae (since the image on the retina almost corresponds with the object, though of less dimensions, and in an inverted position*). If, on the other hand, a point in the circle be seen stereoscopically, the rays A G and B H, which cut each other in D, fall on parts of the retinae almost corresponding, since in this particular case, indeed, the arcs E G and O H are absolutely equal; and therefore the images on the retinae will almost entirely coincide. In complicated cases the geometrical coincidence is not so approximately exact, but in any case it is very evident that the rays that unite in a point in the circle fall on points of the retinae of the two eyes far more nearly correspondent than those belonging to a point in the curve at right angles to the circles. And if, in any position of the apparatus and the eyes, the sections are no longer a circle and conic section, a similar observation will always enable us to determine which of the intersections of the two cones we shall see stereoscopically†.

* The above observations appear to contradict the views advanced by Mr. W. B. Rogers (American Journal, vols. xx. and xxi. 1855 and 1856), in a memoir which only came to my knowledge after I had written the above paper. In this memoir there is a very complete investigation of the appearance presented by very dissimilar drawings when united by means of a simple stereoscope of suitable construction. In mentioning different stereoscopic drawings, it is stated (vol. xxi. p. 176) that two equal circular arcs which are convex) (or concave () to each other, unite themselves stereoscopically into a hyperbolic arc. This, therefore, is analogous to our seeing a parabola (ellipse or hyperbola) in the experiment described in this paper. This is in general correct, if only the arcs are not too great in proportion to the visual angle, since, when the arcs are considerably curved, the experiment requires eyes much practised in the use of the stereoscope, and even then it is effected with difficulty. Two complete circles, moreover, appear, as far as these experiments extend, never to be seen united in this way; and it is certain that if the mind has the choice to conceive the impressions united in different ways, it *prefers* that arrangement which unites stereoscopically the impressions on the points of the retinae which most nearly correspond. If, therefore, on account of this fact, the remarks made in this essay are deprived of complete generality, the same circumstance seems, on the other hand, to confirm the general principle, that it is difficult to unite stereoscopically two images that belong to very different points of the retinae; especially since the above-mentioned experiment of Mr. Rogers's seems to require a greater exertion than most other stereoscopic experiments.

That, moreover, only one stereoscopic image ought in general to be anticipated from more complex drawings (if the left-hand drawing were not sometimes observed with the right eye, and conversely) hardly needs to be mentioned, since it obviously must be so.

† Fig. 7 shows that the rays seen at the same time are necessarily in different planes. While, for instance, the eye B sees the point H, that is to say, while the cylinder passes through the position OH, the eye A sees the point H', which also lies in the line OH, and it is clear that AH' and BH are not in the same plane.

XLIII. *On a Problem of Double Partitions.*

By A. CAYLEY, *Esq.**

IF $a+b+c+\dots=m$, $\alpha+\beta+\gamma+\dots=\mu$ (the quantities being all positive integer numbers, not excluding zero), then $(a, \alpha) + (b, \beta) + (c, \gamma) + \dots$ is considered as a partition of (m, μ) . And the partible quantity (m, μ) and parts (a, α) , (b, β) , &c. being each of them composed of two elements, such partition is said to belong to the theory of *Double Partitions*. The subject (so far as I am aware) has hardly been considered except by Professor Sylvester, and it is greatly to be regretted that only an outline of his valuable researches has been published: the present paper contains the demonstration of a theorem, due to him, by which (subject to certain restrictions) the question of Double Partitions is made to depend upon the ordinary theory of Single Partitions.

Let the question be proposed, "In how many ways can (m, μ) be made up of the given parts (a, α) , (b, β) , (c, γ) , &c." under the following conditions (which are, it will be seen, necessary in the demonstration of the theorem constituting the solution), viz.

$\frac{a}{\alpha}, \frac{b}{\beta}, \frac{c}{\gamma}$, &c. are unequal fractions, each in its least terms,

and

α, β, γ , &c. each less than $\mu+2$.

The number of partitions is

$$= \text{coeff. } x^m y^\mu \text{ in } \frac{1}{(1-x^a y^\alpha)(1-x^b y^\beta)(1-x^c y^\gamma)\dots}$$

the fraction being developed in ascending powers of x, y .

Considering the fraction as a function of y , it may be expressed as a sum of partial fractions in the form

$$\frac{A(x, y)}{1-x^a y^\alpha} + \frac{B(x, y)}{1-x^b y^\beta} + \frac{C(x, y)}{1-x^c y^\gamma} + \dots,$$

where

$A(x, y)$ is rational in x , rational and integral of degree $\alpha-1$ in y ,

$B(x, y)$ " " " " $\beta-1$ "

$C(x, y)$ " " " " $\gamma-1$ "

&c.

To find $A(x, y)$ we have, when $y=x^{-\frac{a}{\alpha}}$,

$$A(x, y) = \frac{1}{(1-x^b y^\beta)(1-x^c y^\gamma)\dots};$$

* Communicated by the Author.

or what is the same thing, we have

$$\Lambda(x, x^{-\frac{a}{\alpha}}) = \frac{1}{(1 - x^{b - \frac{a\beta}{\alpha}})(1 - x^{c - \frac{a\gamma}{\alpha}}) \dots}$$

This in fact determines $\Lambda(x, y)$; for the right-hand side of the equation may be reduced to the form

$$\frac{\Lambda_0 + \Lambda_1 x^{-\frac{a}{\alpha}} \dots + \Lambda_{\alpha-1} x^{-\frac{(\alpha-1)a}{\alpha}}}{(1 - x^{ab - a\beta})(1 - x^{ac - a\gamma}) \dots},$$

where $\Lambda_0, \Lambda_1 \dots \Lambda_{\alpha-1}$ are rational functions of x : to do this, it is only necessary (taking ω an imaginary α -th root of unity) to multiply the numerator and denominator by

$$\Pi(1 - \omega x^{b - \frac{a\beta}{\alpha}}) \Pi(1 - \omega x^{c - \frac{a\gamma}{\alpha}}) \dots,$$

where Π denotes the product of the factors corresponding to the $\alpha - 1$ values of ω ; the denominator is thus converted into

$$(1 - x^{\alpha(b - \frac{a\beta}{\alpha})})(1 - x^{\alpha(c - \frac{a\gamma}{\alpha})}) \dots,$$

which is of the form in question; and the numerator becomes a rational function of x and $x^{-\frac{a}{\alpha}}$, integral as regards $x^{-\frac{a}{\alpha}}$, and therefore at once expressible in the form in question. And the equation, viz.

$$\Lambda(x, x^{-\frac{a}{\alpha}}) = \frac{\Lambda_0 + \Lambda_1 x^{-\frac{a}{\alpha}} \dots + \Lambda_{\alpha-1} x^{-\frac{(\alpha-1)a}{\alpha}}}{(1 - x^{ab - a\beta})(1 - x^{ac - a\gamma}) \dots},$$

remains true if instead of $x^{-\frac{a}{\alpha}}$ we write $\omega x^{-\frac{a}{\alpha}}$; in fact, instead of writing in the first instance $y = x^{-\frac{a}{\alpha}}$, it would have been allowable to write $y = \omega x^{-\frac{a}{\alpha}}$, ω being any α -th root, real or imaginary, of unity. Hence recollecting that $\Lambda(x, y)$ is a rational and integral function of the degree $\alpha - 1$ in y , the equation

$$\Lambda(x, y) = \frac{\Lambda_0 + \Lambda_1 y \dots + \Lambda_{\alpha-1} y^{\alpha-1}}{(1 - x^{ab - a\beta})(1 - x^{ac - a\gamma}) \dots},$$

which is true for the α values $\omega x^{-\frac{a}{\alpha}}$ of y , must be true identically; or this equation gives the value of $\Lambda(x, y)$. And the values of $B(x, y)$, $C(x, y)$, &c. are of course of the like form.

Now consider the term

$$\frac{\Lambda(x, y)}{1 - x^a y^a},$$

where $\Lambda(x, y)$ is a rational and integral function of the degree

$\alpha-1$ in y , and $\frac{a}{\alpha}$ is by hypothesis a fraction in its least terms.

The coefficient therein of $x^m y^\mu$ (the fraction being developed in ascending powers of x, y) is

$$= \text{coeff. } x^m y^\mu \text{ in } \frac{A(x, y)}{1 - x^{\frac{a}{\alpha}} y}$$

(the fraction being developed in ascending powers of x, y). In fact the two fractions only differ by a wholly *irrational* function of x , as is at once obvious by developing $\frac{1}{1 - x^{\frac{a}{\alpha}} y}$ in ascending powers of y . We have, separating the integral part,

$$\frac{A(x, y)}{1 - x^{\frac{a}{\alpha}} y} = U + \frac{A(x, x^{-\frac{a}{\alpha}})}{1 - x^{\frac{a}{\alpha}} y},$$

where U is a rational and integral function of the degree $\alpha-2$ in y . But α being by hypothesis $< \mu+2$, or what is the same thing, $\alpha-2 < \mu$, U does not contain any term of the form $x^m y^\mu$, and therefore

$$\begin{aligned} & \text{coeff. } x^m y^\mu \text{ in } \frac{A(x, y)}{1 - x^{\frac{a}{\alpha}} y} \\ &= \text{do. in } \frac{A(x, x^{-\frac{a}{\alpha}})}{1 - x^{\frac{a}{\alpha}} y}. \end{aligned}$$

And this last is

$$\begin{aligned} &= \text{coeff. } x^m \text{ in } x^{\frac{\mu a}{\alpha}} A(x, x^{-\frac{a}{\alpha}}), \\ &= \text{coeff. } x^{m - \frac{\mu a}{\alpha}} \text{ in } A(x, x^{-\frac{a}{\alpha}}), \\ &= \text{coeff. } x^{\alpha m - a\mu} \text{ in } A(x^\alpha, x^{-a}). \end{aligned}$$

And from the foregoing equation

$$A(x, x^{-\frac{a}{\alpha}}) = \frac{1}{(1 - x^{b - \frac{a\beta}{\alpha}})(1 - x^{c - \frac{a\gamma}{\alpha}}) \dots},$$

this is

$$= \text{coeff. } x^{\alpha m - a\mu} \text{ in } \frac{1}{(1 - x^{\alpha b - a\beta})(1 - x^{\alpha c - a\gamma}) \dots}.$$

The last-mentioned expression is thus the value of

$$\text{coeff. } x^m y^\mu \text{ in } \frac{A(x, y)}{1 - x^a y^\alpha};$$

and hence, *Theorem*,

$$\begin{aligned} & \text{coeff. } x^m y^\mu \text{ in } \frac{1}{(1-x^a y^\alpha)(1-x^b y^\beta)(1-x^c y^\gamma) \dots} \\ &= \text{coeff. } x^{\alpha m - a\mu} \text{ in } \frac{1}{(1-x^{\alpha b - a\beta})(1-x^{\alpha c - a\gamma}) \dots} \\ &+ \text{coeff. } x^{\beta m - b\mu} \text{ in } \frac{1}{(1-x^{\beta a - b\alpha})(1-x^{\beta c - b\gamma}) \dots} \\ &+ \text{coeff. } x^{\gamma m - c\mu} \text{ in } \frac{1}{(1-x^{\gamma a})(1-x^{\gamma b - c\beta}) \dots} \\ &+ \&c., \end{aligned}$$

the fraction of the left-hand side being expanded in ascending powers of x, y , and those on the right-hand side being expanded in ascending powers of x , and the data satisfying the above-mentioned conditions. The number of partitions of (m, μ) is thus found to be equal to the expression on the right-hand side. It is to be noticed that on the right-hand side, when any of the indices $\alpha m - a\mu, \beta m - b\mu, \dots$ is negative, the corresponding coefficient vanishes; and that when the index of the power of x in any factor of a denominator is negative, *e. g.* if $\alpha b - a\beta = -p$, then (in order to develop in ascending powers of x) we must in the place of $\frac{1}{1-x^{\alpha b - a\beta}} = \frac{1}{1-x^{-p}}$ write $\frac{x^p}{x^p - 1} = -\frac{x}{1-x^p}$, and develop in the form $-(x^p + x^{2p} + x^{3p} + \dots)$. The right-hand side is thus seen to be the sum of a series of positive or negative numbers, each of which *taken positively* denotes the number of the single partitions of a given partible number into given parts.

If, using a term of Professor Sylvester's, we say that

$$\text{coeff. } x^m \text{ in } \frac{1}{(1-x^a)(1-x^b) \dots}$$

(where $m, a, b, c \dots$ are positive or negative integers, and the fraction is developed in ascending powers of x) is

= Denumerant of m in respect to the elements (a, b, c, \dots) , say

= Denum^t ($m; a, b, c \dots$).

Then when $m, a, b, c \dots$ are positive, but not otherwise, Denumerant ($m; a, b, c \dots$) denotes the number of ways in which m can be made up of the parts $a, b, c \dots$. And the foregoing result shows that the number of ways in which (m, μ) can be made up of the parts $(a, \alpha), (b, \beta), (c, \gamma), \&c.$ is equal to the sum

$$\begin{aligned} & \text{Denum}^t (\alpha m - a\mu; \alpha b - a\beta, \alpha c - a\gamma, \dots) \\ &+ \text{Denum}^t (\beta m - b\mu; \beta a - b\alpha, \beta c - b\gamma, \dots) \\ &+ \text{Denum}^t (\gamma m - c\mu; \gamma a - c\alpha, \gamma b - c\beta, \dots) \\ &+ \&c. \end{aligned}$$

But, as appears from what precedes, a denominator may be equal to zero, or may denote a number of partitions taken negatively; and it is not allowable, in the place, *e. g.*, of the first denominator, to write *simpliciter*, number of partitions of $am - a\mu$ in respect of $ab - a\beta$, $ac - a\gamma$, &c. The notion of a Denominator is, in fact, an important generalization of the notion of a number of partitions.

2 Stone Buildings, W.C.,
October 4, 1860.

XLIV. On a System of Algebraic Equations.

By A. CAYLEY, Esq.*

THE determination of a, b, c from the system of equations

$$a^2 + bc = \lambda,$$

$$b^2 + ca = \mu,$$

$$c^2 + ab = \nu,$$

in the case where λ, μ, ν have the values 16, 17, and 18 respectively, is the problem known as Colonel Titus's Arithmetical Problem. See Masères' 'Tracts on the Resolution of affected Algebraic Equations,' Lond. 1800. If for shortness we put

$$\sigma = \nu - c^2,$$

then the third equation gives $b = \frac{\sigma}{a}$; and substituting this value of b in the two other equations, we have

$$a^2 + \frac{\sigma c}{a} = \lambda,$$

$$\frac{\sigma^2}{a^2} + ca = \mu;$$

or what is the same thing,

$$a^3 - \lambda a + \sigma c = 0.$$

$$ca^3 - \mu a^2 + \sigma^2 = 0,$$

And from these equations, eliminating a , we have

$$\sigma^4 - 3c^2\sigma^3 + (3c^4 - 2\lambda\mu)\sigma^2 - c^2(c^4 - \lambda\mu)\sigma + c^4\lambda\mu - c^2(\lambda^3 + \mu^3) + \lambda^2\mu^2 = 0,$$

where $\sigma = \nu - c^2$. The equation in c^2 is thus of the fourth order; and in like manner, if instead of c^2 we take σ as the unknown quantity, and substitute therefore for c^2 its value $\nu - \sigma$, the equation in σ will be also of the fourth order. And effecting the

* Communicated by the Author.

reduction, this equation is

$$8\sigma^4 - 12\nu\sigma^3 + (6\nu^2 - 2\lambda\mu)\sigma^2 + (\lambda^3 + \mu^3 - \nu^3 - \lambda\mu\nu)\sigma + (\nu\lambda - \mu^2)(\nu\mu - \lambda^2) = 0.$$

It may be remarked that if $\sigma=0$, then a or b vanishes; and therefore, from the original equations, $\nu\lambda - \mu^2=0$, or $\nu\mu - \lambda^2=0$, which agrees with the result afforded by the foregoing equation in σ . Again, if $\sigma=\nu$, then $c=0$; and therefore, from the original equations, $\nu^2 - \lambda\mu=0$. The left-hand side of the equation in σ , writing therein $\sigma=\nu$, should therefore contain the factor $\nu^2 - \lambda\mu$; its value in fact is $\nu^4 - 2\lambda\mu\nu^2 + \lambda^2\mu^2$, or $(\nu^2 - \lambda\mu)^2$.

If in the original equations we write $a=\frac{x}{z}$, $b=\frac{y}{z}$, the equations become

$$\begin{aligned}x^2 + cyz - \lambda z^2 &= 0, \\y^2 + czx - \mu z^2 &= 0, \\(c^2 - \nu)z^2 + xy &= 0,\end{aligned}$$

which are three homogeneous equations of the second order; from which, if the variables x, y, z are eliminated, we have the required equation in c . And it would not, I think, be difficult, from the known formula for the general case, to deduce the foregoing result corresponding to the very particular case which is here in question.

2 Stone Buildings, W.C.,
September 25, 1860.

XLV. On the Volumetric Determination of Antimony.

By R. SCHNEIDER*.

IN most analytical processes antimony is separated as sulphuret of antimony. As this body varies in composition, and cannot therefore be directly determined, it is necessary to convert it into some form in which it is easily estimated. As far as ordinary quantitative analysis is concerned, this question may be considered as settled; Bunsen's method† of changing sulphuret of antimony into antimoniate of oxide of antimony leaves nothing to be desired for accuracy and certainty. Hitherto,

* Translated from Poggendorff's *Annalen*, vol. cx. p. 634.

† Bunsen's method consists in precipitating the antimony in the usual way by sulphuretted hydrogen, and oxidizing the sulphide of antimony by means of strong nitric acid of spec. grav. 1.524, instead of ordinary nitric acid. By another modification which he adopted, the sulphide was oxidized by being heated with a large excess of oxide of mercury. For the details of the process the reader is referred to the *Chemical Gazette* for 1858, page 311, which contains a full translation of Bunsen's original memoir.

—EDS.

however, there has been no method for directly determining sulphuret of antimony by the method of volumetric analysis. This is attempted to be solved by the following process. The sulphide of antimony, whether precipitated by sulphuretted hydrogen from solutions containing antimonic acid or oxide of antimony, is decomposed by boiling hydrochloric acid in such a manner that for every equivalent of antimony three equivalents of sulphuretted hydrogen are liberated. Consequently the determination of this gas is a measure for the antimony, and its accurate estimation affords an indirect method of determining that body. Hence the real point is an accurate volumetric estimation of sulphuretted hydrogen. This can be effected in several ways.

The determination of sulphuretted hydrogen liberated from sulphide of antimony by treatment with boiling hydrochloric acid, by passing it into solution of sesquichloride of iron, and then estimating the protoxide of iron formed by means of permanganate of potash, gives very inaccurate results. The error is caused by the fact that part of the sulphur which separates from the sulphuretted hydrogen is oxidized by the excess of sesquichloride of iron to sulphuric acid, in consequence of which some protoxide of iron is formed, which is added to that reduced by the sulphuretted hydrogen. H. Rose observed a long time ago, that when sulphuretted hydrogen was passed into a warm solution of sesquichloride of iron, a small quantity of sulphuric acid was formed. The present case offers these conditions; for the solution of sesquichloride becomes heated by the hydrochloric acid vapours which pass into it. The quantity of sulphuric acid formed is never considerable; it varies with the temperature, and small quantities of lower oxides of sulphur appear also to be produced. In no case can the resulting protoxide of iron serve as an accurate measure for the sulphuretted hydrogen.

More accurate results are obtained when the sulphuretted hydrogen is determined by means of iodine.

The accuracy of this method has been doubted on many hands. Bunsen himself* does not appear to consider it particularly accurate, and Mohr† obtained results differing with the concentration of the solutions employed.

It must be admitted that the direct measurement of sulphuretted hydrogen by means of solution of iodine is by no means so accurate as that of sulphurous acid by the same process; yet, when certain precautions are used, the method gives accurate results. These precautions are, that the solution of sulphuretted hydrogen must be greatly diluted, that water free from air must

* Bunsen, *Iodometrische Bestimmungen*, p. 26.

† *Lehrbuch der Titrimethode*, vol. i. p. 302.

be used in the dilution, and that the determinations must be made as rapidly as possible. The transient red colour produced on the addition of iodine to a sulphuretted hydrogen solution containing starch, is only feeble when the solution is greatly diluted; it rapidly disappears, and does not prevent the true iodine reaction.

The details of the process are as follows:—

The sulphide of antimony which has been precipitated by sulphuretted hydrogen from a solution containing tartaric acid*, is collected on a filter of Swedish paper and completely washed out, the last washings being with hot water.

The filter with the partially dried precipitate is carefully removed from the funnel, and pressed into such a form that it can be conveniently introduced into the neck of a small flask. With a little practice this is effected without breaking the filter, and without any loss; it is of course easier when the precipitate is quite dry, for then it occupies less room.

In decomposing the sulphide of antimony and collecting the sulphuretted hydrogen, an apparatus may be used similar to that which Bunsen has described in his paper on iodometric determinations. The size of the flask varies with the quantity of sulphide of antimony: for quantities of 0.3 to 0.4 gm. SbS_3 , a flask of 100 cubic centims. capacity is sufficient; for 0.4 to 1 gm. SbS_3 , the capacity may be 200 cubic centims. The bottom of the flask must be circular, the neck sharply tapering off, long, narrow, and cylindrical. The retort which serves as receiver must have two large bulbous enlargements blown in the neck: the bulb part of the retort must be filled with water freed from air; and from 30 to 50 cubic centims. of caustic ammonia must be added, according to the quantity of the sulphide of antimony.

The sulphide of antimony is then introduced into the flask, hydrochloric acid mixed with one-fifth its volume of water added in excess, the gas delivery-tube affixed, and then the distillation commenced.

The liquid in the receiver must still be alkaline when the distillation is complete. It is left in the retort until cold, and is then placed in a measuring vessel and its volume made up to a litre or to half a litre with boiled water, according to the quantity of sulphuret of antimony taken.

An aliquot part of this, one-fifth or one-tenth, is used for the determination. It is poured into a beaker and diluted with its own volume (or twice its volume) of water free from air; a strip

* The addition of tartaric acid is quite essential; for the sulphide of antimony precipitated from a hydrochloric acid solution persistently retains some chloride of antimony, which is not the case in the presence of tartaric acid.

of litmus paper is placed in the liquid, and dilute sulphuric acid added until the mixture is feebly acid. Solution of starch is added, and the determination, by means of iodine, proceeded with in the usual manner.

If x is the quantity of sulphuretted hydrogen corresponding to 1 cubic centim. of solution of iodine, t the number of cubic centimetres of the solution of iodine used, the value for the antimony is obtained from the equation

$$x = \frac{\text{Sb}}{3\text{HS}} \cdot at.$$

To test the method, 0.326 grm. of pure antimony-glance was decomposed by hydrochloric acid, and the determination made in the above manner.

$$a = 0.000803, \quad t = 123.5.$$

Calculated.

71.48 per cent. antimony

28.52 „ sulphur

100.00

Found.

71.74 per cent.

A higher degree of accuracy than this is obtained by collecting the sulphuretted hydrogen gas in a solution of arsenite of soda, and determining the rest of the arsenious acid by solution of iodine.

Mohr was the first to propose the combination of an alkaline solution of arsenious acid with iodine, and he prefers this in many respects to Bunsen's method. I cannot unconditionally agree with him. If any alteration is to be made in Bunsen's original method, the only reasons for which would be the great dilution required, and the changeability of the sulphurous acid, hyposulphite of soda ought to be preferred to arsenite of soda. The reasons urged by Mohr against its use are not perfectly tenable. That its decomposition by chlorine is different to its decomposition by iodine is unimportant, as it never comes in contact with free chlorine, but always with free iodine; the consumption of iodide of potassium which it occasions is the less to be regarded, as comparatively small quantities of substance are used in iodometric determinations; that its acid solution changes is equally unimportant, as it is only in contact with acid during the short time of the determination. On the other hand, the poisonous properties of arsenious acid are a formidable objection to its use for volumetric purposes, so long as suitable substitutes can be obtained.

For the determination of sulphuretted hydrogen, arsenious acid is well adapted. It affords a precision which is scarcely attainable with any other means.

In using it the above method may be modified as follows.

The same apparatus is used for decomposing the sulphide of antimony and collecting the sulphuretted hydrogen. A solution of arsenious acid, prepared by dissolving arsenious acid (purified by resublimation) in water, pure soda being added until a neutral or feebly alkaline reaction is set up, is placed in the receiver. The solution is so strong that 1 cubic centim. contains about 0.005—0.006 grm. of arsenious acid, and it is compared with a solution of iodine which contains a known quantity of iodine*. According to the quantity of sulphide of antimony, 50, 100, or 200 cubic centims. are used. The latter quantity would be quite sufficient for the sulphuretted hydrogen disengaged from 1.5 grm. sulphuret of antimony.

From the hydrochloric acid gas which passes along with the liberated sulphuretted hydrogen into the receiver the liquid soon becomes acid, and then sulphuret of arsenic is deposited. The decomposition of the sulphuretted hydrogen is rapid and complete. Chloride of antimony does not distil over if the distillation be not continued too long. Nor are any organic substances formed by the action of the boiling hydrochloric acid on the paper, which might subsequently exert a reducing action on the solution of iodine†.

As soon as the liquid in the retort is cold, it is transferred to a measuring-flask, some solution of tartaric acid is added‡, and the flask filled up to the mark. After this liquid has been filtered an aliquot part is measured off, and after saturation with bicarbonate of soda the quantity of arsenious acid is determined by solution of iodine.

The calculation of the result is very simple. If the volume of solution of iodine corresponding to the arsenical solution taken be called V , the volume of iodine solution corresponding to the arsenical solution after the distillation be called o , and the

* This is best effected by adding to a known quantity of the arsenical solution a few drops of hydrochloric acid till it is feebly acid, and then excess of bicarbonate of soda, some solution of starch, and finally solution of iodine.

† A paper filter, 2 inches in diameter, was boiled in hydrochloric acid, and the vapour disengaged collected in water to which 8 cubic centims. solution of arsenic (10 cubic centims. = 23.3 cubic centims. solution of iodine) had been added. After the distillation, the arsenical solution (after the addition of bicarbonate of soda and of starch) required 18.7 cubic centims. solution of iodine. Now $10 : 8 = 23.3 : 18.64$.

‡ The error in the volume caused by the suspended sulphide of arsenic is so small, that in the majority of cases, and where unusually large quantities of substance are not operated upon, it may be neglected.

The sulphide of arsenic precipitated from a hydrochloric acid solution persistently retains some chlorine, doubtless as chloride of arsenic, even after washing. This is not the case when the solution contains tartaric acid.

quantity of iodine contained in 1 cubic centim. of solution of iodine be called a , the quantity of antimony $x = \frac{\text{Sb}}{21} (V - v)a$.

The following determinations show the applicability of the method.

(1) 0.200 grm. pure antimony-glance (from Arnsberg) were directly measured.

10 cubic centims. arsenical solution = 23.5 solution of iodine. $V = 235$ cubic centims. $v = 184.8$ cubic centims. $a = 0.006$ grm.

From which $x = 0.14625$ grm. antimony.

Calculated.	Found.	
71.48 per cent. antimony	71.33 per cent.	[ference.
28.52 ,, sulphur	28.67 ,,	from the dif-
<hr/> 100.00	<hr/> 100.00	

(2) 0.325 grm. pure antimony-glance was decomposed by hydrochloric acid, the strongly diluted solution was mixed with tartaric acid, the antimony precipitated by sulphuretted hydrogen, and the sulphide of antimony measured.

10 cubic centims. arsenical solution = 23.3 cubic centims. solution of iodine.

100 cubic. centims. arsenical solution were employed.

$V = 233$ cubic centims. $v = 151$ cubic centims. $a = 0.006$ grm.

From which $x = 0.239$ grm. antimony.

Calculated.	Found.
71.48 per cent. antimony	71.66 per cent.
28.52 ,, sulphur	28.34 ,,
<hr/> 100.00	<hr/> 100.00

(3) 0.407 grm. chemically pure antimony was dissolved in hydrochloric acid with the addition of some nitric acid; tartaric acid was added, and the sulphide of antimony precipitated from the diluted solution was measured.

10 cubic centims. arsenical solution = 18.25 solution of iodine.

150 cubic centims. arsenical solution were taken.

$V = 273.75$ cubic centims. $v = 160.5$ cubic centims. $a = 0.00758$ grm.

Taken.	Found.
0.407	0.4065 (=99.87 per cent.).

It scarcely needs mention that this method may advantageously be used in determining the *sulphur* in those compounds which are decomposable by hydrochloric acid.

Berlin, July 1860.

XLVI. Illustrations of Symmetrical Integration. By the Rev. ROBERT CARMICHAEL, *Fellow and Tutor of Trinity College, Dublin**.

1. **L**ET the partial differential equation

$$px^2 + qy^2 = kz^2$$

be proposed for integration.

Writing this equation in full, we get

$$x^2 \frac{dz}{dx} + y^2 \frac{dz}{dy} = kz^2,$$

which is obviously equivalent to

$$\frac{dz}{d\left(\frac{1}{x}\right)} + \frac{dz}{d\left(\frac{1}{y}\right)} = -kz^2,$$

and this again to

$$\frac{d\left(\frac{1}{z}\right)}{d\left(\frac{1}{x}\right)} + \frac{d\left(\frac{1}{z}\right)}{d\left(\frac{1}{y}\right)} = k:$$

but the solution of this equation is at once

$$\frac{1}{z} = \frac{k}{2} \left(\frac{1}{x} + \frac{1}{y} \right) + u_0 \left(e^{\frac{1}{x}}, e^{\frac{1}{y}} \right),$$

where u_0 is an arbitrary homogeneous function of the order zero ; or

$$\frac{1}{z} = \frac{k}{2} \left(\frac{1}{x} + \frac{1}{y} \right) + \phi \left(\frac{1}{x} - \frac{1}{y} \right),$$

where ϕ is any arbitrary function.

Similarly, the integral of the partial differential equation in three independent variables,

$$x^2 \frac{dw}{dx} + y^2 \frac{dw}{dy} + z^2 \frac{dw}{dz} = kw^2,$$

is

$$\frac{1}{w} = \frac{k}{3} \left(\frac{1}{x} + \frac{1}{y} + \frac{1}{z} \right) + u_0 \left(e^{\frac{1}{x}}, e^{\frac{1}{y}}, e^{\frac{1}{z}} \right).$$

More generally, the integral of the partial differential equation in n independent variables,

$$x^m \frac{du}{dx} + y^m \frac{du}{dy} + z^m \frac{du}{dz} + w^m \frac{du}{dw} + \&c. = ku^m,$$

* Communicated by the Author.

is

$$\frac{1}{w^{m-1}} = \frac{k}{n} \left(\frac{1}{x^{m-1}} + \frac{1}{y^{m-1}} + \frac{1}{z^{m-1}} + \&c. \right) + u_0 (e^{\frac{1}{x^{m-1}}}, e^{\frac{1}{y^{m-1}}}, e^{\frac{1}{z^{m-1}}}, \&c.).$$

2. The method employed in solving the primary type, in examples of this species, suggests the possibility of the instantaneous integration of such partial differential equations as

$$p \cos^2 x + q \cos^2 y = k \cos^2 z;$$

or, more generally, taking three independent variables,

$$\cos^2 x \cdot \frac{dw}{dx} + \cos^2 y \cdot \frac{dw}{dy} + \cos^2 z \cdot \frac{dw}{dz} = k \cos^2 w; \quad (\alpha)$$

or, again, such equations as

$$(a^2 - x^2)^{\frac{1}{2}} \cdot \frac{dw}{dx} + (b^2 - y^2)^{\frac{1}{2}} \cdot \frac{dw}{dy} + (c^2 - z^2)^{\frac{1}{2}} \cdot \frac{dw}{dz} = k(m^2 - w^2)^{\frac{1}{2}}; \quad (\beta)$$

or

$$(1 + x^2) \cdot \frac{dw}{dx} + (1 + y^2) \cdot \frac{dw}{dy} + (1 + z^2) \cdot \frac{dw}{dz} = k(1 + w^2). \quad (\gamma)$$

These equations, in fact, are respectively reducible to the forms

$$\frac{d \cdot \tan w}{d \cdot \tan x} + \frac{d \cdot \tan w}{d \cdot \tan y} + \frac{d \cdot \tan w}{d \cdot \tan z} = k, \quad (\alpha')$$

$$\frac{d \cdot \sin^{-1} \frac{w}{m}}{d \cdot \sin^{-1} \frac{x}{a}} + \frac{d \cdot \sin^{-1} \frac{w}{m}}{d \cdot \sin^{-1} \frac{y}{b}} + \frac{d \cdot \sin^{-1} \frac{w}{m}}{d \cdot \sin^{-1} \frac{z}{c}} = k, \quad (\beta')$$

$$\frac{d \cdot \tan^{-1} w}{d \cdot \tan^{-1} x} + \frac{d \cdot \tan^{-1} w}{d \cdot \tan^{-1} y} + \frac{d \cdot \tan^{-1} w}{d \cdot \tan^{-1} z} = k; \quad (\gamma')$$

the solutions of which equations are respectively

$$\tan w = \frac{k}{3} (\tan x + \tan y + \tan z) + u_0 (e^{\tan x}, e^{\tan y}, e^{\tan z}), \quad (\alpha'')$$

$$\sin^{-1} \frac{w}{m} = \frac{k}{3} \left(\sin^{-1} \frac{x}{a} + \sin^{-1} \frac{y}{b} + \sin^{-1} \frac{z}{c} \right) + u_0 (e^{\sin^{-1} \frac{x}{a}}, e^{\sin^{-1} \frac{y}{b}}, e^{\sin^{-1} \frac{z}{c}}), \quad (\beta'')$$

$$\tan^{-1} w = \frac{k}{3} (\tan^{-1} x + \tan^{-1} y + \tan^{-1} z) + u_0 (e^{\tan^{-1} x}, e^{\tan^{-1} y}, e^{\tan^{-1} z}). \quad (\gamma'')$$

3. Let it be proposed to integrate the partial differential equation

$$\frac{p}{x} + \frac{q}{y} = \frac{z}{y^2}.$$

The solution of this equation is, by the ordinary operational

350 The Rev. R. Carmichael's *Illustrations of*
method, in its full expanded form,

$$z = u_0(e^{x^2}, e^{y^2}) \cdot \left\{ 1 + \frac{\log y}{1} + \frac{(\log y)^2}{1 \cdot 2} + \frac{(\log y)^3}{1 \cdot 2 \cdot 3} + \&c. \right\},$$

which may be instantly condensed into the shape

$$z = y \cdot \phi(x^2 - y^2).$$

In the same manner it may be shown that the integral of the equation

$$\frac{1}{x} \cdot \frac{dw}{dx} + \frac{1}{y} \cdot \frac{dw}{dy} + \frac{1}{z} \cdot \frac{dw}{dz} = \frac{w}{z^2}$$

is

$$w = z \cdot \phi(x^2 - y^2, y^2 - z^2, z^2 - x^2).$$

4. The following partial differential equation, which was originally suggested by Laplace, has been discussed by Sir John Herschel (*Transl. of Lacroix*, p. 690),

$$\frac{dz}{dx} + \frac{y}{z} \frac{dz}{dy} = x^m z^n.$$

Now if we multiply this equation by x , and convert the dependent variable, the equation becomes obviously

$$\left(x \frac{d}{dx} + y \frac{d}{dy} \right) \cdot \frac{-1}{(n-1)z^{n-1}} = x^{m+1},$$

and the solution is at once

$$-\frac{1}{(n-1)z^{n-1}} = \frac{x^{m+1}}{m+1} + u_0(x, y).$$

More generally, the integral of the equation

$$\frac{dw}{dx} + \frac{y}{x} \frac{dw}{dy} + \frac{z}{x} \frac{dw}{dz} = x^m y^n z^p w^q$$

is

$$-\frac{1}{(q-1)w^{q-1}} = \frac{x^{m+1} y^n z^p}{m+n+p+1} + u_0(x, y, z).$$

5. If it be required to integrate the symmetrical partial differential equation

$$\frac{dz}{dx} \cdot \frac{dz}{dy} = pq = kz,$$

we find by the method of Lagrange, as perfected by Charpit,

$$z = k^{\frac{1}{2}}(x + \alpha)(y + \phi \alpha),$$

where α is an arbitrary constant, and ϕ any arbitrary function.

Upon inspection of the solution now found, and consideration of the mode in which the partial differential coefficients derived

from it satisfy the given equation, and the characters of these *dérivées*, we see at once that, similarly, the solution of the partial differential equation in three independent variables,

$$\frac{d^2u}{dx\,dy} \cdot \frac{d^2u}{dz\,dx} \cdot \frac{d^2u}{dy\,dz} = ku,$$

is

$$u = k^{\frac{1}{3}}(x + \alpha)(y + \phi\alpha)(z + \chi\alpha),$$

χ being a new arbitrary function.

Similarly, we see that the solution of the partial differential equation in four independent variables,

$$\frac{d^3u}{dx\,dy\,dz} \cdot \frac{d^3u}{dw\,dx\,dy} \cdot \frac{d^3u}{dz\,dw\,dx} \cdot \frac{d^3u}{dy\,dw\,dz} = ku,$$

is

$$u = k^{\frac{1}{4}}(x + \alpha)(y + \phi\alpha)(z + \chi\alpha)(w + \psi\alpha),$$

where ψ is a third new arbitrary function.

6. Let it be required to determine the values of u and v , being given the system of simultaneous partial differential equations,

$$\left. \begin{aligned} a_1 D_x u + b_1 D_y u + c_1 D_z u + a_2 D_x v + b_2 D_y v + c_2 D_z v &= k_1 u + k_2 v, \\ a_1 D_x v + b_1 D_y v + c_1 D_z v + a_2 D_x u + b_2 D_y u + c_2 D_z u &= k_1 v + k_2 u, \end{aligned} \right\}$$

where the coefficients a_1, b_1, c_1 , &c. are supposed to be constants.

These equations may be thrown into the form

$$\left. \begin{aligned} (a_1 D_x + b_1 D_y + c_1 D_z - k_1)u + (a_2 D_x + b_2 D_y + c_2 D_z - k_2)v &= 0, \\ (a_1 D_x + b_1 D_y + c_1 D_z - k_1)v + (a_2 D_x + b_2 D_y + c_2 D_z - k_2)u &= 0, \end{aligned} \right\}$$

whence we derive

$$(a_1 D_x + b_1 D_y + c_1 D_z - k_1)^2 \cdot u - (a_2 D_x + b_2 D_y + c_2 D_z - k_2)^2 \cdot u = 0.$$

This equation may evidently, in general, be reduced to the form

$$(\alpha_1 D_x + \beta_1 D_y + \gamma_1 D_z - 1)(\alpha_2 D_x + \beta_2 D_y + \gamma_2 D_z - 1) \cdot u = 0,$$

where $\alpha_1, \beta_1, \gamma_1$, &c. are known constants. And the solution required is

$$u = \Phi_1(e^{\frac{x}{\alpha_1}}, e^{\frac{y}{\beta_1}}, e^{\frac{z}{\gamma_1}}) + \Psi_1(e^{\frac{x}{\alpha_2}}, e^{\frac{y}{\beta_2}}, e^{\frac{z}{\gamma_2}}),$$

where Φ_1, Ψ_1 are arbitrary homogeneous functions of the first degree.

The value of v is then to be had by substitution of the expression for u just found, in either of the given equations.

Trinity College, Dublin,

October 1860.

XLVII. *The Dichroscope.* By H. W. DOVE*.

[With a Plate.]

THE apparatus to which I have given the above name is intended for the following purposes:—

1. To represent interferences, and spectra in different-coloured lights, both separately and combined.

2. To imitate the phenomena of dichroism both in the case in which the dichroitic crystals are viewed through a double-refracting arrangement, as, for example, Haidinger's dichroitic lens, and also in the case of the phenomena produced when the dichroitic crystals themselves are used as analysers in a polarizing arrangement.

3. To combine elliptically, circularly, and rectilinearly polarized and unpolarized light, not in such a manner that the one is produced by the polarizing, and the other by the analysing arrangement, but so that they traverse the doubly refracting media simultaneously, and are then submitted to any analysing arrangement.

ab, fig. 1, Plate IV. is the three-sided brass prism of my polarizing apparatus (*Farbenlehre*, page 202), moveable in a brass case on an ordinary telescope-stand, in horizontal and vertical directions. At one end there is the lens with the polarizing mirror *cd*, and at the other end at *a* the analysing Nicol with the ocular. *hegf* is the dichroscope, which can be placed in one of the ordinary slides which carry the other arrangements, in which case these other arrangements (the polarizing Nicol and the circular-polarizing mica plate) may be placed on one side.

The dichroscope is a four-sided brass box 81 millims. long, 75 millims. high, and 70 millims. broad. The posterior side of the box represented in the figure is closed, and in the middle of this side there is a cylindrical piece in which a rod is inserted. This rod is attached either to a piece of glass *ef*, or to a moveable glass disc, which can thus be exchanged for one another. The piece of glass or the disc are turned by means of a knob which projects from the outside of the box, after the end of the rod which reaches out of the cylindrical piece has been tightened by a screw, but not so as to prevent rotation. In the open sides of the box *hf* and *fg* coloured glasses can be inserted, while *he*, when other slides are not used, is destined to receive cooled glasses or crystals, or a large rotating circular-polarizing plate of mica. The sides *hf* and *gf* can be closed by slides; and slides can be placed in *he* which have a longitudinal slit for prism-

* Translated from Poggendorff's *Annalen*, vol. cx. p. 265.

experiments, or a circular aperture for grating-experiments. Two mirrors belong to the apparatus, 108 millims. long and 60 millims. broad, one of which is silvered, and the other blackened. Either of these mirrors can be placed at g in the polarizing angle, by means of a cleft in the direction ge ; in this case the polarizing mirror cd is removed. For the future the mirror at g will be designated by cd .

The apparatus is intended either for ordinary daylight, or for sunlight directly incident on cd at the polarizing angle.

In order to simplify the phenomena of the different combinations, I assume that the linear-analysing arrangement is so placed, that in a calc-spar cut at right angles to the axis the rings with the black cross are visible. It is assumed that the ocular is at right angles. If the plate of a body with feeble double refraction, or if a plate of crystal with a large axial angle is to be viewed, the polarizing microscope (*Farbenlehre*, p. 209) is used as an analysing arrangement. It is so arranged that the mica plate which serves for circular or elliptical analysis can be used in the same manner as the ordinary ocular. If, on the contrary, cooled or pressed glasses, or crystals, are viewed beyond the distance of perceptible vision, the ocular is removed on one side, and the ordinary analysing Nicol is used.

The following combinations are obtained:—

1. cd silvered mirror, ef the glass disc. Natural light reaches the analysing arrangement from cd , and linear-polarized from ef .

(a) fg closed by the slide; white light linear-polarized.

(b) fg closed by the slide, in hf coloured glass; according to the nature of the glass, monochromatic or polychromatic linear-polarized light.

(c) Without slide and without coloured glass; white natural light and white linear-polarized light, consequently partially polarized, the rings scarcely visible. If, after looking for some time at the rings with the black cross, the slide at fg is rapidly removed, the complementary rings with the clear cross are seen at first.

(d) Without slide, the coloured glass at fg ; combination of unpolarized coloured light with white linear-polarized light. The cross brightly coloured in the colour of the glass; the rings in white light are somewhat changed by the uniform coloured light.

(e) The coloured glass at hf without slide; coloured polarized light with white unpolarized. The rings almost invisible, on account of the preponderating white light.

(f) Different-coloured glasses at hf and gf . The system of rings appears as if the analysing arrangement had been turned 90° ; the cross is coloured, and the rings appear as changes from

the colour of the two glasses; for the parts which are dark in homogeneous light are illuminated by the coloured light from *fg*.

(*g*) If a large rotating mica plate be placed at *eh*, the corresponding combinations of circular and elliptical light with unpolarized light are obtained.

2. The silvered mirror is replaced by the polarizing mirror. There reach the analysing arrangement two masses of light linearly polarized in the same plane, or if the mica plate is placed at *he*, circularly or elliptically polarized. They are,

(*a*) Both white or both coloured, if at *hf* and *gf* there are either no glasses at all or both of the same colour (as in 1 *a, b*.)

(*b*) White and coloured, if at *hf* or *fg* there is a coloured glass, by which the white preponderates so greatly that the action of the coloured almost disappears.

(*c*) Different-coloured, if at *hf* and *hg* there are glasses of different colours, in which case, strictly speaking, the plane glass must not be parallel to the mirror, but each must be inclined at the maximum polarizing angle which corresponds to that of the colour.

If with a polarizing apparatus without a dichroscope the light of a white flame be concentrated on the polarizing Nicol, and if a piece of cobalt glass 6 millims. thick be placed before the eye, the blue and red rings in carbonate of lead are obtained quite distinct*, but intersecting one another; in calc-spar splendid alternations of deep-red, blue, and violet concentric circles are obtained. By adding a green glass the blue rings may be isolated, by adding a red glass the red rings. But cobalt glasses which completely extinguish the middle of the spectrum are extremely rare; and the darkening of the light is so great, that in

* Both in this case, and also where, instead of a thick cobalt glass, blue and red are combined in the dichroscope, it may appear surprising that the dark rings in blue light appear much more elongated in the direction of the line joining the middle points of both systems than those in red light, although the axial angle in red light is greater than in blue light. The reason of this phenomenon is at once seen from the prism analysis; the spectrum of the flame seen through the cobalt glass appears to consist of two masses of light separated by a dark space,—of which the red is homogeneous, for the form of the slit is very distinct; while on the contrary the blue mass of light extends over a larger space, and passes from light blue to dark blue. The rings produced by this light are not simple, but appear like the circular waves which are formed by drops falling in water after one another in a straight line. Just as in this case two straight waves somewhat inclined to each other result from the simultaneously produced elementary waves, so in the case of the light, dark lines are formed which accordingly appear rectilinear on the side, while at one end they are limited by a flatter curve than at the other. This formation becomes distinct if a green glass is combined with the cobalt glass. With prism analysis the blue mass of light appears narrower, and in the polarizing arrangement the rings were rounded.

apparatus with feeble light the phenomenon is greatly attenuated, and in ordinary daylight is not apparent, for the red rings then quite disappear. Dichromatic combinations by superposing different-coloured glasses are impossible with many colours, for a pure red and a pure green glass become then quite opaque. These evils are obviated by the arrangement (*c*). By combining monochromatic and dichromatic glasses, which are placed at *hf* and *fg*, any desired union of colours for polychromatic illumination may be obtained, which by covering either *hf* or *fg* are immediately decomposed into their components. Indeed, by combining a red and a green glass, Newton's rings with white light at once appear.

To make this more apparent, I have given in figs. 2, 3, 4, 5 the appearance of two crossed, plane, polished gypsum crystals in red, yellow, green, and blue light, in which the white lights in the figure must be considered as appearing in the corresponding colour of the dark interference lines. If two of these figures are imagined to be superposed, we obtain the phenomenon of the combined illumination. As the square which is common to both the gypsums shows five interference lines in red and seven in blue, the alternation of red and blue lines is then directly apparent, for the diagonals of both colours appear black. A very instructive phenomenon is obtained, if, while *fg* is covered by the slide, only one gypsum is examined and at *hf* a glass is inserted, half of which allows one colour to pass, and the other half the other colour. This sharp removal of the interference lines is especially manifest. If it be desired to show the part which each colour plays in the phenomenon with white light, a glass is chosen for *hf*, of which one half is colourless and the other half coloured.

I know no transparent bodies which transmit homogeneous yellow light. Another arrangement was accordingly made for this colour. The mirror *cd* was removed, the polarizing glass substituted for the glass plate at *ef*, and a condensing lens placed at *b*. Near its focus was a spirit-lamp coloured yellow by common salt, and immediately behind this a white flame; between these two there was a glass plate intended to colour this flame. Since light of a different colour penetrates a homogeneous flame, the desired combination is attained in this manner.

In the place of well-polished gypsums, cooled glasses or compensators of rock-crystal may be used.

In order to extend the results obtained for interferences where the same spaces are traversed with unequal velocities to interferences where different spaces are traversed with the same velocities, the dioptric grating may be used.

I have shown (Poggendorff's *Annalen*, vol. xxvi. p. 310) that, on looking at a brightly illuminated aperture on a flame through two crossed glass gratings held close before the eye, there is seen the splendid phenomenon of the spectra distorted in the form of quadrants, which Fraunhofer has depicted in the sixth plate of his grating-experiments.

These obliquely distorted spectra may be shown to be connected with the apparent deflection of light depending on the length of the wave, by interposing a monochromatic, dichromatic, or tri-coloured glass,—in which cases the unchanged figures of the aperture at regular distances are seen to form systems of quadrants with mutual edges; in the first case there is one, in the second two, and so on. In using the dichroscope, the circular aperture is in a slide at *h e*. It is viewed from the distance of distinct vision, through gratings rotating on each other. The alternate closing of *h f* and *g f* gives the components.

In all these interference experiments, it is of course desirable to know the homogeneous colours which prevail in the combined action of the glasses. This is effected if the circular aperture is changed for a narrow slit, and if a strongly refracting flint-glass prism is used instead of the crossed or simple grating. In this manner the component spectra and the result of their combination are obtained.

3. Between the silvered mirror *c d* and the plane glass *e f* the rotating mica plate is inserted.

In this manner are obtained combinations of circularly or elliptically polarized light with light linearly polarized by *e f*, which reach the eye from *g f*.

(a) If, without coloured glasses, the light incident through *g f* is right-circularly polarized, and that through *h f* linearly polarized, the distorted cross in the plate of calc-spar shows that the light passing out is right-elliptically polarized light.

(b) If, without coloured glasses, the light through *g f* is left-circular, it gives with the linearly-polarized light incident through *h f* left-elliptically polarized light.

(c) If by turning the mica plate the light through *g f* is right- or left-elliptical, it remains elliptical even if combined with the right- or left-linearly polarized, but it approximates more towards linear, and becomes so when the azimuth of the principal section is 0° .

(d) If a coloured glass is inserted so as to colour either the linear or the circular or elliptical light, the white light preponderates so as to make it appear that the figure of the circular or elliptical or that of the linear is seen in white light.

(e) If, on the contrary, two coloured glasses are inserted, the succession of colours on the rings of the straight quadrant appears different and removed from those in the crooked one,

while the cross becomes distorted and appears of the colour of the circular (elliptical) light.

4. The parallel glass is replaced by the polarizing glass, and the silvered mirror $c\ d$ inserted at g .

(a) The glass is so placed that the light entering at fg and then polarized by refraction is equal in intensity to that which enters at hf , and is then polarized at right angles to it by refraction. No figure is formed on the plate of calc-spar; the light is unpolarized.

(b) The slide is gradually pushed forward at fg ; and by the gradually increasing difference between the masses of light polarized at right angles to each other, the unpolarized light passes through the middle stage of partially polarized into completely polarized. The phenomenon is the same as if two flat prisms of tourmaline, whose edges are parallel to the axis of the crystal, were gradually pushed one over the other so as to form a plate becoming continually thicker.

(c) The slide at hf is gradually pushed forward after the one at gf has been removed. The phenomenon obtained appears as if two prisms of topaz (smoky quartz) parallel to the axis were superposed on one another.

(d) Coloured glasses are placed in hf and fg ; the phenomena of dichroitic crystals are obtained as follows:—

(α) The plate of calc-spar is removed, and the Nicol exchanged for a doubly refracting achromatic prism. When a round aperture is inserted at he , two figures of it are obtained in different colours, which by rotating the analysing prism, pass into one another. This is the dichroitic lens. By using a Nicol, a picture is seen to change its colour. If glasses of the same colour are placed at hf and gf , and if the glass piece is arranged so that the refraction and reflexion of the polarized masses of light are of unequal intensity, two figures of the same colour, but of unequal intensity, are obtained, and the emergent light is partially polarized. This represents the crystals which are improperly termed dichroitic, but which are so far connected with them that, when used as an analysing arrangement, they produce the phenomena of tourmaline to a feebler extent. If the intensity and the colour are equal, the arrangement represents the plate of a doubly refracting crystal which has no dichroitic properties.

(β) The calc-spar plate is inserted; and the phenomena are obtained which dichroitic crystals exhibit when they are used in the polarizing apparatus as an analysing arrangement. Strictly speaking, that is the polarizing arrangement in this case which is the analysing in the other, and *vice versa*; but since, from the law of reciprocity, one arrangement results directly from the

other, I have preferred to retain the same arrangement of the apparatus as in the preceding case.

We shall most readily appreciate the phenomena which now appear, if that which is obtained in the calc-spar plate with a certain-coloured light and with coincident planes of reflexion of the polarizing and analysing mirror be considered as applied to the phenomena which this plate exhibits when it is looked at in light of another colour, the mirrors being crossed. The rings are a combination of two coloured systems of rings with a light and a dark cross, and which therefore appears brightly coloured in the colour corresponding to the bright cross. The succession of colours of the rings is consequently highly peculiar, and is very distinct when well-annealed glasses are examined. If it be desired to imitate the phenomena seen when a dichroite, dichroitic mica, a rubellite, repidolite, or smoky quartz is employed as analysing arrangement, we have merely to use the proper coloured glasses. In the first case the cross appears blue; in the second it appears red with preponderating green rings; in rubellite the cross is the red of the alpine rose; in repidolite deep grey and almost black, as in dark smoky quartz, while the rings appear brownish yellow. What we shall obtain by turning the Nicol 90 degrees is manifest if we imagine that the phenomenon of the dark cross for one colour is changed into the light cross, and, on the contrary, the phenomenon of the light cross for the other colour is changed into that of the dark cross. It is especially beautiful when an achromatic prism of calc-spar is used as analysing arrangement with a red and blue glass. The systems of rings, one with the blue and the other with the red cross, then partially intersect one another.

An idea is obtained of the colours which result from the combination of the system of rings, if a longitudinal slit is inserted on the side towards the eye, and this is viewed through a prism of rock-crystal. By the intermixture of both spectra, colours are obtained which would not be expected from the components, corresponding to the investigations of Wünsch and Helmholtz. The active components are, however, obtained in the most different intensities if a rotating Nicol is interposed between the prism of rock-crystal and the eye.

(γ) The arrangement remains the same, but a rotating mica plate of one-quarter difference of path is inserted at *eh*. Right or left circular or elliptical light of one colour is seen to combine with right or left circular or elliptical light of another colour. The appearances here presented give a key to the complicated phenomena seen in the polarizing apparatus when the circular-polarizing mica or gypsum plate is changed for one which has a much greater difference of path.

The conditions for circular polarization, that is, equal intensity of two masses of light polarized at right angles to each other, whose difference of phase is an odd multiple of a quarter undulation, can, as is well known, be satisfied in two ways,—either by two total internal reflexions in a single-refracting body, or by refraction in a double refractor. The condition of equal intensity is satisfied in the first case if the azimuth of the plane of reflexion with the primitive plane of polarization is plus or minus 45° , and in the second case if that of the principal sections is $\pm 45^\circ$. Fresnel's rhombohedron serves for total reflexion, or if the light is to remain in the axis of the instrument, my reversion prism (*Farbenlehre*, p. 240). In total reflexion

the difference of phase after n reflexions is $\frac{n}{8}$; using double-refracting bodies, it is proportional to the thickness of the plate, and to its double-refracting power. Airy accordingly splits a plate of mica* until it has the required difference of phase, while in Babinet's compensator the change is gradually produced by wedges of rock-crystal moved gradually over each other. In both cases with an equal double-refracting power the thickness changes. The thickness, on the contrary, remains the same with a change in the double-refracting power if, as I have shown, circular polarization is produced by pressing or warming a glass plate between the polarizing and analysing arrangement; but since the length of waves is different for different colours, the condition of a determinate difference of phase can only be simultaneously satisfied for a determinate colour; and it follows directly from the formulæ for both the rays separated by double refraction, that with increasing thickness of the plate the difference in this respect between the various colours increases in a corresponding degree; so that the same arrangement in one part of the linear-polarized spectrum changes the light into circularly polarized light, in another into linearly polarized light, in others into light polarized at right angles, with all transitions through right and left elliptical. I have shown this in the experiments on circular polarization by allowing, by means of a rotating prism, the individual parts of the spectrum to pass over the aperture of a polarizing Nicol. In this case the system of rings in calc-spar, and then all changes of form corresponding to the various stages of polarization, are seen to pass in single colours, from which the very complicated phænomenon in white light finds a direct explanation. The dichroscope gives another derivation of the same phænomenon. For if a plate of mica is inserted at *he*, a blue glass at *hf*, and a red glass at *gf*, on covering

* Darker uses a plate of gypsum, and a combination of several in the beautiful apparatus which he calls the *retarding slide*.

fg the system of rings in the quadrant is seen in right-circularly (elliptically) polarized light; if *hf* is covered, the rings in the quadrant in red light are seen in left-circular (elliptical) light; by removing the slide, the cross whose arms are on one side of a different colour than the other.

The polarizing apparatus which I have described gives an objective representation of the phenomena. It is simply necessary to concentrate an intense light on the aperture of the polarizing Nicol through an objective, and to remove the ocular from the analysing nearer to the polarizing Nicol. The light emerging from the analysing Nicol is caught on a white surface, on which the rings are represented in corresponding size. I have not applied this to the dichroscope, for in fact the darkening of the objective pictures is considerable when a deeply-coloured glass is held before the eye which is examining it.

The dichroscope can be combined with Nörremberg's apparatus in the following manner. For the composition of the interference colours, a plate must be added parallel to the polarizing plate; the coloured glasses stand then one over the other in the same vertical plane. For imitating the dichroitic phenomena, the glass plates are placed so that these planes of reflexion are at right angles to each other. On account of the difficulty of the illumination, the apparatus can only be used immediately in the neighbourhood of the window, which, on account of the side light, greatly limits the intensity of the colours of polarization. In this respect an apparatus is to be preferred which, as it can be directed towards the source of light like a telescope, can be used as well by day as by night in any part of a room. When it is placed parallel to the earth's axis, it can be used as a sun-clock even in twilight, according to a process described in Poggendorff's *Annalen*, vol. xxxv. p. 596,—an application which I made (*Maas und Messen*, p. 62) in 1845, ten years before these apparatus appeared under the special name of Polar-clocks.

Several of the dichroscopes which I have described have been very neatly made by Langhoff, the philosophical-instrument maker.

XLVIII. *Notes on Atmospheric Electricity.*

By Professor W. THOMSON, F.R.S.*

TWO water-dropping collectors for atmospheric electricity were prepared, and placed, one at a window of the Natural Philosophy Lecture-room, and the other at a window of the

* Communicated by the Author, having been read before the British Association, June 1860.

College Tower of the University of Glasgow. A divided ring-electrometer was used at the last-mentioned station; an electrometer adapted for absolute measurement, nearly in the form now constructed as an ordinary house electrometer, was used in the lecture-room. Four students of the Natural Philosophy Class, Messrs. Lorimer, Lyon, M'Kerrow, and Wilson, after having persevered in preliminary experiments and arrangements from the month of November, devoted themselves with much ardour and constancy during February, March, and April to the work of observation. During periods of observation, at various times of day, early and late, measurements were completed and recorded every quarter minute or every half minute,—the continual variations of the phenomenon rendering solitary observations almost nugatory. During several hours each day, simultaneous observation was carried on on this plan at the two stations. A comparison of the results manifested often great discordance, and never complete agreement. It was thus ascertained that electrification of the air, if not of solid particles in the air (which have no claim to exclusive consideration in this respect), between the two stations and round them, at distances from them not very great in comparison with their mutual distance, was largely operative in the observed phenomena. It was generally found that after the indications had been negative for some time at both stations, the transition to positive took place earlier by several minutes at the tower station (upper) than at the lecture-room (lower). Sometimes during several minutes, preceded and followed by positive indications, there were negative indications at the lower, while there were only positive at the upper. In these cases the circumambient air must have contained negative (or resinous) electricity, if a horizontal stratum of air several hundred feet thick overhead, containing as much positive electricity per cubic foot as there must have been of negative per cubic foot of the air about the College buildings on those occasions, would produce electrical manifestations at the earth's surface similar in character and amount to those ordinarily observed during fair weather.

Beccaria has remarked on the rare occurrence of negative atmospheric indications during fair weather, of which he can only record six during a period of fifteen years of very persevering observation by himself and the Prior Ceca. On some, if not all, of those occasions there was a squally and variable wind, changing about rapidly between N.E. and N.W. On several days of unbroken fair weather in April and May of the present year the atmospheric indication was negative during short periods, and on each occasion there was a sudden change of wind, generally from N.E. to N.W., W., or S.W. For instance, on the 3rd of May, after a warm, sunny, and very dry day, with a gentle N.E.

breeze and slight easterly haze in the air, I found about 8.30 P.M. the expected positive atmospheric indication. After dark (nearly an hour later) it was so calm that I was able to carry an unprotected candle into the open air and make an observation with my portable electrometer. To my surprise I found a somewhat strong negative indication, which I observed for several minutes. Although there was no sensible wind in the locality where I stood*, I perceived by the line of smoke from a high chimney at some distance that there was a decided breeze from W. or S.W. A little later a gentle S.W. wind set in all round, and with the aid of a lantern I found strong positive indications, which continued as long as I observed. During all this time the sky was cloudy, or nearly so. That reversed electric indications should often be observed about the time of a change of wind may be explained, with a considerable degree of probability, thus:—

The lower air up to some height above the earth must in general be more or less electrified with the same kind of electricity as that of the earth's surface; and, since this reaches a high degree of intensity on every tree-top and pointed vegetable fibre, it must therefore cause always more or less of the phenomenon which becomes conspicuous as the "light of Castor and Pollux," known to the ancients, or the "fire of St. Elmo" described by modern sailors in the Mediterranean, and which consists of a flow of electricity of the kind possessed by the earth into the air. Hence in fair weather the lower air must be negative, although the atmospheric potential, even close to the earth's surface, is still generally positive. But if a considerable area of this lower stratum is carried upwards into a column over any locality by wind blowing inwards from different directions, its effect may for a time predominate, and give rise to a negative potential in the air, and a positive electrification of the earth's surface.

If this explanation is correct, a whirlwind (such as is often experienced on a small scale in hot weather) must diminish, and may reverse, the ordinary positive indication.

Since the beginning of the present month I have had two or three opportunities of observing electrical indications with my portable electrometer during day thunder-storms. I commenced the observation on each occasion after having heard thunder, and I perceived frequent impulses on the needle which caused it to vibrate, indicating sudden changes of electric potential at the place where I stood. I could connect the larger of these impulses with thunder heard some time later, with about the same degree of certainty as the brighter flashes of lightning during a thunder-storm by night are usually recognized as distinctly connected with distinct peals of thunder. By counting time I estimated the distance of

* About six miles south of Glasgow.

the discharge not nearer on any occasion than about four or five miles. There were besides many smaller impulses; and most frequently I observed several of these between one of the larger and the thunder with which I connected it. The frequency of these smaller disturbances, which sometimes kept the needle in a constant state of flickering, often prevented me from identifying the thunder in connexion with any particular one of the impulses I had observed. They demonstrated countless discharges, smaller or more distant than those that gave rise to audible thunder. On none of these occasions have I seen any lightning. The absolute potential at the position of the burning match was sometimes positive and sometimes negative; and the sudden change demonstrated by the impulses on the needle were, so far as I could judge, as often augmentations of positive or diminutions of negative, as diminutions of positive or augmentations of negative. This afternoon, for instance (Thursday, June 28), I heard several peals of thunder, and I found the usual abrupt changes indicated by the electrometer. For several minutes the absolute potential was small positive with two or three abrupt changes to somewhat strong positive, falling back to weak positive, and gathering again to a discharge. This was precisely what the same instrument would have shown anywhere within a few yards of an electrical machine turned slowly so as to cause a slow succession of sparks from its prime conductor to a conductor connected with the earth.

I have repeatedly observed the electric potential in the neighbourhood of a locomotive engine at work on a railway, sometimes by holding the portable electrometer out a window of one of the carriages of a train, sometimes by using it while standing on the engine itself, and sometimes while standing on the ground beside the line. I have thus obtained consistent results, to the effect that the steam from the funnel was *always negative*, and the steam from the safety-valve always positive. I have observed *extremely strong* effects of each class from carriages even far removed from the engine. I have found strong negative indications in the air after an engine had disappeared round a curve, and its cloud of steam had dissolved out of sight.

In almost every part of a large manufactory, with steam-pipes passing through them for various heating purposes, I have found decided indications of positive electricity. In most of these localities there was some slight escape of high-pressure steam, which appeared to be the origin of the positive indications.

These phenomena seem in accordance with Faraday's observations on the electricity of steam, which showed high-pressure steam escaping into the air to be in general positive, but negative when it carried globules of oil along with it.

XLIX. *Experimental Researches on the Laws of Absorption of Liquids by Porous Substances.* By THOMAS TATE, Esq.*

[With a Plate.]

THE ascent of liquids in porous substances has hitherto been regarded by physicists as similar to, if not identical with, the ascent of liquids in capillary tubes; but the following researches show that the former phenomena are regulated by peculiar and distinctive laws. I was first led to this inquiry by observing the large portion of water which had been evaporated, in the course of a day, from a vase of flowers. Vegetable and animal life are, no doubt, intimately connected with the laws regulating the absorption of liquids by porous substances.

The following general laws have been derived from the results of these experiments.

In an atmosphere saturated with the vapour of the liquid,

(1) The rate of diffusion varies inversely as the space through which the liquid of absorption has moved.

(2) The rate of diffusion, at equal distances from the surface of the liquid with which the absorbent is in contact, is equal in all directions, that is to say, the rate of diffusion is independent of the force of gravity.

(3) The rate of diffusion, other things being the same, increases with the temperature.

(4) The liquid diffuses itself equally over the surface of the absorbent; that is, the weight of the liquid absorbed by a unit of surface is everywhere the same.

(5) In equal times the force of absorption performs the same amount of *work* or dynamic effect.

(6) If an absorbent, saturated with moisture, be exposed to a dry atmosphere, the evaporation goes on for the most part uniformly. And so on to other laws, which will be hereafter illustrated.

1. *Rate of diffusion of liquids through absorbents in an atmosphere saturated with the vapour of the liquid.*

The absorbents employed in these experiments were strips of unsized paper, calico, linen, &c., of various thicknesses and texture, and also thin columns of plaster of Paris. The liquids used were distilled water, turpentine, linseed oil, alcohol, solutions of starch, together with solutions of different salts, &c. The line formed by the liquid in its ascent on the strips of unsized paper is so distinct and sharply defined as to enable us to determine, with the greatest precision, the distance of this line from the level surface of the liquid. The strips of paper, calico,

* Communicated by the Author.

linen, &c., were prepared for experiment in the following manner: after being cut to their desired widths, and in some cases graduated, stout platinum wires were attached to their lower extremities, to give them a uniform tension and to sink these extremities in the liquid; they were then steeped in boiling distilled water, and afterwards thoroughly dried by evaporation; and immediately before being used they were suspended for about an hour in the atmosphere, saturated with the vapour of the liquid. The ascent of the liquid was, for the most part, observed by means of a cathetometer; but in some cases the strip itself was graduated. The space *S*, moved over by the liquid through the pores of the absorbent, is the elevation of the liquid line on the absorbent measured from the level of the surface of the liquid in which the absorbent is immersed. This liquid line was sometimes best observed by transmitted light, at other times by reflected light. Plate IV. fig. 8 represents the apparatus which was, for the most part, employed in these experiments:—

AB a glass jar having a welt at the top ground smooth to receive the ground brass plate *CD*; *S* a stuffing-box attached to the plate, through which passes the stout wire *FG*, supporting the absorbent *GK* immersed in the liquid *BK*; *E* a stopcock communicating, when required, with the stopcock of an air-pump; *LM* a water-bath maintained (when required) at a constant temperature by means of a jet of steam. The sliding wire, *FG*, enables us to raise or depress the absorbent as may be required; and the stopcock *E* and ground plate enable us to keep the absorbent suspended in an atmosphere saturated with the vapour of the liquid, or it may be in a vessel exhausted of air. The apparatus was modified to suit peculiar circumstances. In experimenting with such substances as turpentine, or with linseed oil, the vessel *AB* was exhausted of air by means of the air-pump, and then the external air was allowed to enter the vessel through a tube filled with dry chloride of calcium. The absorbent having been for some time suspended in this dry air, its lower extremity, *K*, was immersed in the oil by depressing the sliding wire *FG*.

Experiment I.

The liquid used in this experiment was water. The absorbent was unsized paper, 2 inches in width, close in texture, and weighing .72 gr. per square inch. The temperature was 56° throughout the experiment.

The experimental value of the velocity of ascent, *v*, in the third column, is found by dividing the increment of space .25 by the mean interval of time; thus we find corresponding

$$\text{to } S=2, v = \frac{.25}{\frac{1}{2}(15.2-9.2)} = .083.$$

Ascent of liquid in inches, S.	Corresp. time in minutes, T.	Velocity of ascent per minute, v.	Value of v by formula $v = \frac{1}{6S}$.	Value of v by formula $v = \frac{S}{2T}$.	Value of T by formula $T = 3S^2$.
0	0	0
·75	1·75	1·68
1·00	3·16	·166	·166	·016	3·00
1·25	4·75	4·68
1·50	6·82	6·75
1·75	9·20	9·18
2·00	12·00	·083	·083	·083	12·00
2·25	15·20	15·18
2·50	18·66	18·75
3·00	27·00	·055	·055	·055	27·00
3·25	31·50	31·68
3·50	36·50	36·75
4·00	47·50	·045	·042	·042	48·00
4·25	53·00	54·18
4·50	59·00	60·75
4·75	67·00	67·00
5·00	75·00	·031	·033	·033	75·00
5·50	90·50	90·75
6·00	108·00	·026	·027	·027	108·00
6·50	128·50	126·75

It will be observed how very nearly the velocity, as determined by the formula $v = \frac{1}{6S}$, coincides with the velocity found by experiment. Hence we conclude that *the velocity of ascent of the liquid through the absorbent varies inversely as the space.*

The relation between T and S may be generally expressed by the formula

$$T = \alpha(S + \beta)^2 - \rho, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where α , β , and ρ are constants.

By differentiation, we get

$$\frac{dT}{dS} = \frac{1}{2\alpha(S + \beta)};$$

but

$$v = \frac{dS}{dT};$$

$$\therefore v = \frac{1}{2\alpha(S + \beta)}; \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and when $\beta = 0$, or is very small, as in the foregoing experiment, we have

$$v = \frac{1}{2\alpha S}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

From equations (1) and (2) we get

$$v = \frac{S + \beta}{2(T + \rho)}. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Obs. In accordance with observations before made, the value of the velocity derived from the formula $v = \frac{S}{2T}$ only holds true when T is large compared with ρ .

Nearly the same results were obtained with an absorbent of one-half the width. Hence it appears that *the rate of diffusion is not dependent upon the width of the absorbent.*

This experiment was repeated with the vessel AB exhausted of air. The results were nearly the same as those above recorded. Hence it would appear that *the rate of diffusion is not affected by the presence of air.*

Rate of evaporation.—In order to determine the rate of evaporation from the surface of an absorbent saturated with moisture, the absorbent of this experiment was taken out of the water and suspended in the air from the scale of a delicate balance, the dry bulb of the thermometer being 62° , and the wet bulb 58° . The weight of the whole water absorbed was found to be 24.3 grs., that is, 1.215 gr. per square inch of surface. The weight lost during each successive interval of 30 minutes was found to be very nearly 4.74 grs. At the end of $2\frac{1}{2}$ hours the rate of evaporation sensibly declined, and the last few grains of moisture were retained with considerable tenacity. Hence it appears that *the rate of evaporation, from the surface of an absorbent saturated with moisture, is for the most part uniform.*

An absorbent thus placed in contact with water (as will be hereafter more fully shown) becomes saturated with moisture; and when it is exposed to the action of the atmosphere, it becomes equally dry at all parts in the same time. But if the whole surface of the absorbent be plunged into the water and then suspended in the air, the lower portion of the absorbent retains a sensible film of water by adhesion to the surface; this portion of the absorbent may be said to be supersaturated with moisture, and it will be found that the higher portion of the absorbent will become dry much sooner than the lower, and also that the rate of evaporation will not go on uniformly.

The absorbent of this experiment, after being dried, was suspended in contact with water as usual, and exposed to the air. For the first 4 inches of the ascent of the liquid through the absorbent, the law of ascent closely corresponded with that of the foregoing experiment; but after this the rate of ascent rapidly decreased, until the liquid had risen to the height of about 8 inches, where it appeared to become stationary. At this time the dry bulb of the thermometer indicated $65^{\circ}.5$, and the wet bulb 62° , giving a difference of $3^{\circ}.5$. When the difference between the indications of the two bulbs had become 5° , the liquid stood at the height of 9 inches; and when this differ-

ence was only $2^{\circ}5$, the liquid stood at the height of 7.6 inches; in this manner the liquid fell and rose according as the air contained more or less moisture.

The results of the two foregoing observations will be more fully considered when I describe the construction of a new hygrometric instrument.

The following experiment was made to determine the rate of diffusion under an increase of temperature.

Experiment III.

The absorbent was the same as that used in Experiment II. The temperature of the water-bath, LM, was maintained at 110° .

Ascent of liquid in inches, S.	Corresp. time in minutes, T.	Velocity of ascent per minute, v.	Value of v by formula $v = \frac{1}{3.64 S}$	Value of $v = \frac{S}{2T}$ by experim.	Value of T by formula $T = 1.82 S^2$.
0	0	0
.75	1.00	1.02
1.00	1.75	.277	.275	.285	1.82
1.25	2.80	2.54
1.50	4.08	4.09
1.75	5.60	5.57
2.00	7.30	.138	.137	.137	7.28
2.25	9.20	9.21
2.50	11.50	11.37
3.00	17.00	.096	.092	.089	16.38
3.25	19.60	19.22
3.50	22.80	22.29
4.00	29.50	.067	.068	.067	29.12
4.25	33.26	32.87

The results of this experiment, on being compared with those of Experiment II., show that *the rate of diffusion is considerably increased by an increase of temperature.*

Hastings, October 18, 1860.

[To be continued.]

L. Note on *Transcendental Roots*.

By JAMES COCKLE, M.A., F.R.A.S., F.C.P.S. &c.*

FILLING up that part of my "Sketch," &c. in the last August Number which relates to a cubic, and making

$$b = \sqrt{1 - a^2},$$

from

$$x = \phi a, \quad \frac{dx}{da} = -\frac{\phi b}{3b},$$

* Communicated by the Author.

we deduce by differentiation

$$\begin{aligned}\frac{d^2x}{da^2} &= -\frac{a\phi b}{3b^3} - \frac{1}{3b} \cdot \frac{d\phi b}{db} \cdot \frac{db}{da} \\ &= -\frac{a\phi b}{3b^3} - \frac{1}{3b} \left(-\frac{\phi a}{3a} \right) \left(-\frac{a}{b} \right).\end{aligned}$$

This becomes, on reduction,

$$\frac{d^2x}{da^2} - \frac{a}{1-a^2} \cdot \frac{dx}{da} + \frac{x}{3^2(1-a^2)} = 0,$$

a linear differential equation, the integral of which is

$$x = A \sin \left(\frac{\sin^{-1} a}{3} + B \right).$$

Substitute this value of x in the given cubic. The result must vanish identically and independently of a ; and from the relations

$$A^3 - 4A = 0, \quad A^3 - 8 = 0, \quad \sin 3B = 0,$$

we infer

$$x = 2 \sin \left(\frac{\sin^{-1} a + 2n\pi}{3} \right).$$

The unsymmetric trinomial form

$$x^n - nx + n(n-1)a = 0$$

is in some respects more convenient than that employed in the "Sketch," and, through

$$fx \cdot \frac{dx}{da} - \frac{x}{n} \cdot \frac{dfx}{da} = 0,$$

it leads to

$$n^{n-1} a^{n-1} \left(\frac{dx}{da} \right)^n = \left(\frac{dx}{da} + 1 \right)^{n-1} \left(\frac{dx}{da} - n + 1 \right).$$

Each unsymmetric form indeed leads to a derived equation, which, by properly combining it with the given equation, may be made *linear* in x .

I might have supported an early objection (S. 3. vol. xxxv. p. 436) which I took to a portion of Mr. Jerrard's investigations, by citing a paper* by the late Thomas White, of Dumfries Mathematical Academy, which (received April 1816) was printed in

* "On the Algebraical Expansion of Quantity, by Division and Evolution; and on the Symbol $\sqrt{-1}$, which is usually considered to denote impossible or imaginary Quantity." White (p. 61), in reference to the series "usually but whimsically . . . denominated *neutral*," says that "all ambiguity vanishes when it is considered that this series is finite, and that attention must always be paid to the remainder."

the Appendix to 'The Ladies' Diary' for 1839. Commenting on such an expansion as

$$\frac{1}{1+x} = 1 - x + x^2 - \dots \pm x^{n-1} \mp \frac{x}{1+x},$$

White observes (pp. 60, 61) that "in summing such *finite* serieses, the remainders must *never* be neglected. . . . The introduction of the remainders removes all paradox. Hence, however great n may be supposed, the remainders must *never* be neglected in serieses generated by algebraic division," &c. And I presume that White would have maintained that the equation

$$\frac{1}{1+x} = 1 - x + x^2 - \dots \pm x^{n-1} \mp \text{\&c. in infin.}$$

was always algebraically false, though it might be arithmetically true.

Other doubts (Ibid. p. 437), to which I sought to direct Mr. Jerrard's* attention, he seems to have cleared up (S. 4. vol. iii. p. 457). But†, in leaving unassailed the theory of Abelian's with composite indices, he virtually leaves his own processes without defence from the objections to which that theory gives rise. As observed by M. Kronecker, *all* the distinct values of a cyclical function of $x_0, x_1, \dots x_4$ being given, the five roots x may be deduced from them *rationally*‡. Now Mr. Jerrard's functions Ξ ,

* Mr. Jerrard (Ibid. p. 459) has adverted to some researches of Legendre on cubics, in connexion with which I would call attention to "a few remarks" of James Lockhart "on the roots of the cubic $x^3 - 3x - 1 = 0$, which apply to all cubic equations," and which I communicated to the *Mechanics' Magazine* (vol. lv. p. 173). Lockhart's theorem respecting quintics (Ibid. vol. liii. p. 449; *et vide* lv. pp. 172, 173) may be easily demonstrated by multiplying the quadratic into x , eliminating x^2 , and repeating this process as far as may be necessary.

† M. Kronecker states that every (algebraically) soluble equation of a prime degree μ is an Abelian, if we regard as known a quantity which itself is a root of an Abelian of the $(\mu - 1)$ th degree (see Serret, *Cours*, 1854, p. 564). There is not, as I once suspected, a corresponding proposition when μ is not prime. The solution of the problem for a composite number n is obtained the moment that we have resolved it for the case in which the degree of the Abelian is one of the powers (of a prime number) contained in n (M. Kronecker, *ibid.* p. 566; M. Hermite, *ib.* pp. 569 *et seq.*).

‡ See M. Hermite's Essay, *Sur la Théorie des Equations Modulaires et la Résolution de l'équation du cinquième degré* (Paris, 1859), p. 27; consult also M. Hermite's "Considérations sur la Résolution Algébrique de l'équation du 5^e degré," at pp. 326-336 of vol. i. of the *Nouvelles Annales de Mathématiques*, par. MM. Terquem et Gerono, 1842. I am not aware that these important researches of M. Hermite have been completed. Mr. T. T. Wilkinson informs me that the "*suite*" is not found in any subsequent volume up to the present date.

I may add that, if we had an irreducible non-cyclical *réduite* of the 5μ th

P, and W are cyclical. Consequently he would obtain the general solution of a quintic in a form which involved no quintic radicals. Such a result is of course inadmissible*.

Without reiterating objections which I urged in the last May Number, let me observe that of the two systems (σ_1) and (σ_2) suggested by Mr. Jerrard (S. 4. vol. iii. p. 114), the last is inadmissible. To admit it, would be to ignore the principle (a neglect of which led me into an objection to Wantzel's argument) that interchanges of the constituent symbols do not affect the form of the function. There is no other conclusion than that

$${}^1r_n(x_\alpha)(\alpha\beta) = {}^1r_n(x_\beta), \dots {}^1r_n(x_\alpha)(\alpha\epsilon) = {}^1r_n(x_\epsilon),$$

so that (σ_1) must hold universally. If (σ_2) also holds, (o''') (Ib. p. 115) takes the form

$$(V^3 + r_1(0)V^2 + r_2(0)V + r_3(0))^5 = 0,$$

which is inconsistent with the supposition (Ibid. p. 112) that $V^{15} + C_1V^{14} + \dots = 0$ cannot be depressed. Hence (compare Ib. p. 116)

$$V^3 + B_1V^2 + r_2(x)V + r_3(x) = 0$$

is the form of one of Mr. Jerrard's cubics; and in order to construct it, we have to encounter the difficulty of solving the general equation of the fifth degree, for in no other way can $r(x)$ be determined. Apart, then, from Cauchy's theorem and M. Hermite's argument, Mr. Jerrard's process presents intrinsic objections fatal to its success. Let me add that although, by a theorem of Abel, every value of $\zeta\Xi$ at p. 79 of Mr. Jerrard's 'Essay' be a root of (ac) , so that we may deduce

$$\Xi - r\{P_{f(\beta\epsilon)}\} = 0,$$

degree in t , the function

$$\phi(m) = t_1^m + t_2^m + \dots + t_\mu^m$$

(in which t_1, t_2, \dots, t_μ are the μ values of t formed by permuting all the roots excepting x_0) cannot be symmetric in x for all values of m , otherwise the *réduite* would not be irreducible. Hence m may be so assigned as to render

$$\phi(m) = a + bx_0 + \dots + ex_0^4;$$

and x_0 , which may be found as a rational function of ϕ , can contain no other radicals than those which enter into t_1, t_2, \dots, t_μ .

* The moment we proceed to the practical application of our formulæ, we are led to conclude that in those cases in which the root is expressible by quintic (with or without quadratic) surds, the sextic in θ has a rational linear factor; and that when cubic radicals appear, the given quintic and its resolvent sextic are, each, reducible.

still, if we would reconcile this with the other conclusions of theory, we must give the equation the form

$$\Xi f(\beta \epsilon) - r \{ P f(\beta \epsilon) \} = 0,$$

a result which gives us no aid whatever in our search after a finite root.

4 Pump Court, Temple, London,
September 17, 1860.

LI. *Spectrum-analysis of London Waters.*

By A. and F. DUPRÉ*.

HAVING been lately engaged in the analysis of several London waters, we took occasion to examine them by the recently published method of Bunsen and Kirchhoff; and since this spectrum-analysis adds two new constituents (*i. e.* lithium and strontium) to those already known, the results obtained may not be uninteresting to some of the readers of the *Philosophical Magazine*. If a small portion of the dry residue of any of the waters examined is brought into the flame of an apparatus such as described in Bunsen and Kirchhoff's paper†, the lines Li α and K α are seen with more or less distinctness as soon as the first glare of the sodium and calcium spectrum is somewhat diminished. After the Li, K, and Na have volatilized, the calcium lines come out with increased brilliancy; and if the wire is now dipped into HCl and again brought into the flame, the lines Sr α and Sr γ are seen, as well as a very brilliant calcium spectrum. The strontium lines come out generally with greater brilliancy if the wire, before being moistened with HCl, is held for some time in a reducing flame, easily obtained by closing the air-holes of the Bunsen's burner. In some of the waters, especially the deep-well waters, the line Li α is somewhat masked by the bright sodium and calcium spectra: it is, however, in all cases seen with great distinctness if the residue of the water is treated with sulphuric acid and alcohol in the manner described by Bunsen and Kirchhoff under the head of lithium. The strontium lines may also be seen with great brilliancy on dissolving in hydrochloric acid some of the crust deposited in boilers and kettles, and bringing a drop of the solution into the flame of the apparatus. The shallow waters appear to be rather richer in Li and Sr than the deep-well waters; the presence of Li in the latter can, however, easily be demonstrated in an ounce or even in half an ounce of the water.

The following are the waters examined:—Thames water, taken

* Communicated by the Authors.

† *Phil. Mag.* vol. xx. p. 89.

at low and high tide, Westminster Bridge; also two samples as supplied by the Chelsea and Lambeth Water Companies. Water supplied by the New River Company; and water from the undermentioned wells:—

Duck Island Well, St. James's Park..	} Above the London
Pump H, Lincoln's Inn	
Burnett's Distillery, Vauxhall	} clay.
Whitbread's Brewery, Chiswell Street.	
Guy's Hospital well	} From the sand above
Trafalgar Square well	
	} the chalk.
	} Chalk.

The above waters may be taken to represent the whole of the London supply, since, beside the specimens from the Thames and New River, others from the three principal water-bearing strata of London are included.

To guard against all possible sources of fallacy, the waters were evaporated in platinum vessels, and all filtration avoided. It need scarcely be mentioned that the alcohol, HO , SO^3 , and HCl used were free from lithium and strontium.

LIII. *Chemical Notices from Foreign Journals.*

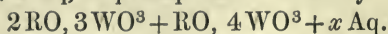
By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from p. 298.]

SCHEIBLER has published* an account of researches on the tungstates, with which he has been engaged during the last six years.

Tungstic acid exists in two distinct modifications—one insoluble in water (ordinary tungstic acid), and a modification soluble in water (metatungstic acid). And there are two corresponding groups of salts.

The tungstates, the salts of the insoluble modification, have been repeatedly investigated. The alkaline salts are somewhat soluble in water; those of the alkaline earths and metals, which are obtained by double decomposition, are insoluble, amorphous or crystalline precipitates. These tungstates are characterized by the fact that, when treated with a stronger mineral or organic acid, a yellow pulverulent or white cascos precipitate of hydrate of tungstic acid is formed, according as the decomposition is effected in the warm or in the cold. To these salts, which have usually been considered acid salts, Scheibler assigns the formula $3\text{RO}, 7\text{WO}^3 + x\text{Aq}$, or perhaps more correctly



* *Ber. der Akad. der Wissensch. zu Berlin*, April 1860.

Tungstate of Soda, $3\text{NaO}, 7\text{WO}^3 + 16\text{Aq}$, crystallized in the cold, and $3\text{NaO}, 7\text{WO}^3 + 14\text{Aq}$, crystallized in the water-bath, forms beautiful large monoclinic prisms.

Tungstate of Potass, $3\text{KO}, 7\text{WO}^3 + 6\text{Aq}$, crystallizes in small, difficultly soluble, scaly crystals.

Tungstate of Lithia, $3\text{LiO}, 7\text{WO}^3 + 16\text{Aq}$, crystallizes in easily soluble, very beautiful large monoclinic prisms.

Metatungstates.—The only salt of this class hitherto described is the ammonia salt discovered by Margueritte.

The solutions of these salts are not decomposed by acids with separation of a yellow or white hydrate of tungstic acid. The metatungstates are formed from the tungstates by adding a strong acid to their solutions as long as the precipitate redissolves, —or better still, by continuously boiling the tungstates with an excess of hydrate of tungstic acid. This hydrate is obtained by the double decomposition of tungstate of soda with chloride of calcium, and digesting the precipitate which is formed with hydrochloric acid.

The metatungstates are very soluble, and crystallize only when the solution is very concentrated. The alkaline salts crystallize in octahedra. The ammonia salt melts in water like phosphorus; its solution is strongly refracting. Its formula is $\text{NH}^4\text{O}, 4\text{WO}^3 + 9\text{Aq}$. The potash and soda salts are similar. On mixing a warm concentrated solution of metatungstate of ammonia with solution of chloride of barium, no precipitate is formed, but on cooling there is deposited

Metatungstate of Baryta, $\text{BaO}, 4\text{WO}^3 + 9\text{Aq}$.—It crystallizes in large fatty crystals, which are a combination of the octahedron with the prism. The crystals have a considerable specific gravity, and are readily soluble in hot water.

When a warm concentrated solution of metatungstate of baryta is treated with an equivalent quantity of sulphuric acid, the liquor, after filtration from the sulphate of baryta, deposits, on evaporation *in vacuo*, small quadratic octahedra of metatungstic acid, which apparently have the composition $2\text{HO}, 4\text{WO}^3 + 7\text{Aq}$. It is a strong acid, and expels nitric and hydrochloric acid from their combinations. Its solution can be boiled for some time without change, and can be evaporated to the consistence of syrup in the water-bath; but if it be further concentrated, it passes into the insoluble modification.

Metatungstic acid is an admirable reagent for nitrogenous bases, which are all precipitated by it in white flakes; it exceeds phosphomolybdic acid in delicacy. Acid solutions which only contain $\frac{1}{200,000}$ of quinine or strychnine are made distinctly turbid, and after twenty-four hours small flakes are deposited on the bottom of the vessel. The free metatungstic acid itself is not

necessary; any of its salts, mixed with a mineral acid, will do equally well.

By double decomposition of the baryta salt with the corresponding sulphates, Scheibler has obtained the magnesia, copper, manganese, nickel, cobalt, zinc, and cadmium salts. The strontia and lime salts were obtained by treating their chlorides with free metatungstic acid.

Metatungstic ether was obtained by treating crystallized tungstate of silver with iodide of ethyle. Besides these tungstates and metatungstates, the author has described some new compounds of oxide of tungsten, obtained partly by new and partly by known methods.

Wöhler* has described some new salts of suboxide of silver.

Molybdate of Suboxide of Silver, $\text{Ag}^2\text{O}, 2\text{MoO}^3$, is a heavy, black, lustrous powder consisting of well-defined regular octahedra. It is obtained by dissolving molybdate of oxide of silver in strong ammonia, and passing hydrogen gas through the solution. The operation succeeds best when the temperature is about 90° . At a higher temperature metallic silver is formed.

Tungstate of Suboxide of Silver, $\text{Ag}^2\text{O}, 2\text{WO}^3$, is prepared in the same manner, and forms a black, crystalline, glittering powder, the particles of which under the microscope appear to possess rhombic faces.

Chromate of Suboxide of Silver.—The reduction of chromate of silver is effected at the ordinary temperature; but it is impossible to obtain the suboxide salt pure, as it always contains an admixture of metallic silver.

The preparation of metallic calcium is effected by M. Caron as follows†. 300 parts of fused and powdered chloride of calcium are mixed with 400 parts of granulated zinc, and 100 parts of sodium in pieces; the whole is placed in a crucible heated to redness; when the action commences, the fire is moderated so as to prevent the volatilization of the zinc, but it is ultimately raised as high as possible.

In this manner a crystalline regulus is obtained containing 10 to 15 per cent. of calcium, the remainder being zinc and a small quantity of iron. To obtain the calcium, this alloy is heated in a carbon crucible in tolerably large pieces. The calcium thus prepared contains traces of iron; it is of a pale yellowish colour; its density was found to be 1.6 to 1.8, but this is somewhat too high. It is not perceptibly volatile; but the zinc in volatilizing carries away traces of it. It keeps toler-

* Liebig's *Annalen*, April 1860.

† *Comptes Rendus*, March 1860.

ably well in dry air, but first becomes covered with a layer of oxide.

It burns with difficulty in the blowpipe flame, because it becomes covered with a layer of lime; but the combustion of its wire gives rise to red scintillations of great beauty.

In the anticipation of obtaining crystallized titanium, Wöhler* fused a mixture of titanous acid with cryolite and a piece of aluminum, together with a flux of chloride of sodium and potassium.

The aluminum was found afterwards in the form of a lamellar porous slag, which, when treated with soda lye, left a quantity of lustrous brownish crystals; by treatment with hydrochloric acid these became quite colourless. The body is a combination of aluminum with titanium and silicon, but not apparently in any definite proportions. It is decomposed by chlorine, forming chloride of titanium, silicon, and aluminum. It is slowly attacked by hydrochloric acid, forming hydrogen gas and oxide of silicon.

Michel†, at Wöhler's suggestion, has prepared a series of compounds of aluminum and the metals.

Tungsten-Aluminum was obtained by melting together 15 parts of tungstic acid with 30 of cryolite, 30 of chloride of potassium and sodium, and 15 of aluminum. On treating the slag with hydrochloric acid, the excess of aluminum was dissolved out, and the compound was left as a grey crystalline powder, containing individual crystals several millimetres in length, which under the microscope were found to consist of rhombic prisms with terminal faces. Their spec. grav. was 5.58, and they were very hard and brittle. Their formula was found to be $Al^4 W$. Treated with caustic soda, all the aluminum was dissolved out, and pure tungsten left.

Molybdenum-Aluminum.—Molybdic acid was dissolved in hydrofluoric acid, and the solution evaporated to dryness, and fused with cryolite, flux, and aluminum in the manner described above. The regulus, by successive treatment with soda and nitric acid, yielded a crystalline powder which was found to consist of iron-grey rhombic prisms. Analysis led to the formula $Mo Al^4$.

Manganese-Aluminum.—Obtained by fusing 10 parts of chloride of manganese, 30 of chloride of potassium and sodium, and 15 of aluminum. The regulus, by treatment with dilute hydrochloric acid, left a quantity of interwoven stellate-group needles, some of them 5 or 6 lines long. The needles were quadratic prisms of the specific gravity 3.402. The compound is easily dissolved by concentrated hydrochloric acid. The analysis

* Liebig's *Annalen*, February 1860.

† Ibid. July 1860.

indicated the formula Mn Al^3 . Part of the manganese was replaced by iron.

Iron-Aluminum.—Obtained by fusing aluminum with protochloride of iron, and the alkaline flux. The regulus was very crystalline, and on careful treatment left the compound in fine hexagonal prisms. The analysis pointed to the formula Fe Al^2 .

Nickel-Aluminum.—Obtained in a similar manner to the above compounds, in large crystalline scales of a tin-white colour. Its specific gravity is 3.647. It is readily soluble in concentrated hydrochloric acid. Its composition is Ni Al^6 .

M. Michel has obtained *titanium-aluminum* in microscopic quadratic plates; his analysis led to the formula Ti Al^3 .

Heintz has communicated* some experiments of Richter on the preparation of artificial boracite. 200 grms. of a mixture of chloride of sodium and chloride of magnesium were mixed with 5 grms. of a compound of boracic acid and magnesia, obtained by precipitating a boiling solution of sulphate of magnesia and borax by carbonate of soda. To the mixture thus prepared 10 grms. of finely powdered boracic acid were added, and the whole, well mixed together, heated in a platinum crucible. The fused mass was heated with dilute hydrochloric acid, which left undissolved a crystalline powder. This powder was seen under the microscope to consist of two kinds of crystals, some of a prismatic form, while others were tetrahedra and octahedra. On digesting this mass for several days with hydrochloric acid, the former were dissolved, while the latter were scarcely if at all attacked. These latter were found on analysis to have the composition 6MgO , 8Bo^3 , MgCl , which is the formula of boracite. When heated, they exhibited the pyro-electrical phenomena characteristic of the powder of tourmaline.

The prismatic crystals appear to be a mixture of two compounds, one of which Heintz supposes has the formula MgO Bo^3 , and the other the formula 2MgO Bo^3 .

Nordenskjöld and Chydenius† describe some experiments on the crystallization of thoria.

Thoria, prepared by the ordinary method from orangite, was mixed with four times its weight of borax glass, and placed in a flat platinum dish, which, coated with magnesia, was fitted in an unglazed porcelain vessel, and this again in an ordinary clay crucible, and the whole exposed to the heat of a porcelain furnace. The highest heat of the furnace continued forty-eight hours, and the cooling was very slow.

After the operation the borax glass presented two layers—the

* Poggendorff's *Annalen*, August 1860.

† *Ibid.*

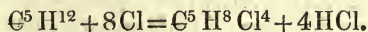
upper a colourless glass, and the lower an opaque white mass. In the clear glass and on the sides of the crucible there were interspersed individual brown crystals, which were left undissolved when the whole mass was treated with hydrochloric acid: the white mass also left after this treatment a heavy hard powder, seen under the microscope to consist of crystals like the larger ones.

A crystallographical examination of the larger crystals was made. Though small, they were frequently found as well developed as the most perfect crystal models. Their form was that of a dimetric prism with a pyramid. Hence, if this is correct, the oxide of thorium is isomorphous with oxide of tin and titanitic acid. It is a binoxide, ThO^2 , and not ThO and Th^2O^3 , as has hitherto been supposed.

A similar experiment was made with tantalic acid, by fusing it with microcosmic salt under the same circumstances. The slag contained a crystalline powder consisting of tantalic acid, besides some individual larger needles of the same substance, the form of which on examination was found to be that of a rhombic prism.

In his researches on oxide of amylene*, Bauer obtained hydride of amylene as an accessory product in the preparation of amylene. It is not possible to separate these substances by fractional distillation, as their boiling-points are too near; but by acting on the mixture with bromine, bromide of amylene, $\text{C}^5\text{H}^{10}\text{Br}^2$, is formed, while hydride of amylene remains intact. Bromide of amylene boils at 170° , and hence the latter body is readily separated by fractional distillation.

When chlorine is passed into hydride of amylene, a brisk action is set up, the gas is absorbed, hydrochloric acid gas is liberated, and considerable heat is disengaged. The first product of the action is chloride of amylene, $\text{C}^5\text{H}^{11}\text{Cl}$; when this body, which boils at 102° , is separated by fractional distillation, and chlorine gas passed into the residue for several hours, the liquor becomes very thick: when it is distilled it begins to boil at 180° , and the thermometer rises quickly to 230° , and remains for some time between this point and 240° . It afterwards rises to 300° ; and then decomposes, leaving a black residue. The body which distils between 230° and 240° has the formula $\text{C}^5\text{H}^8\text{Cl}^4$, and is the chloride of trichlorinated amylene formed in accordance with the equation

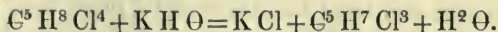


It is a colourless liquid insoluble in water: it is heavier than

* Phil. Mag. vol. xx. p. 44.

water, and burns when heated, with the flame characteristic of chlorine compounds.

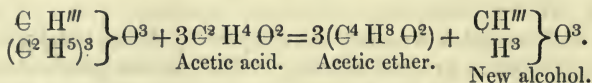
Acted on by potash, chloride of potassium and trichlorinated amylene are formed, thus:—



Trichlorinated amylene boils at about 200° ; in other respects it resembles the chloride of trichlorinated amyle.

Bromine acts on hydride of amyle at 100° , forming similar products.

By the action of sodium-alcohol on chloroform, Kay obtained a body of the composition $\text{C}^7 \text{H}^{16} \Theta^3$, which may be regarded as the ethylic ether of the triatomic alcohol $\left. \begin{array}{c} \text{CH}^{\text{III}} \\ \text{H}^3 \end{array} \right\} \Theta^3$, that is, $\left. \begin{array}{c} \text{C H}^{\text{III}} \\ (\text{C}^2 \text{H}^5)^3 \end{array} \right\} \Theta^3$. Regarding the body in this light, M. Sawitsch* has examined the action of acetic acid upon it, expecting that the reaction would take place in the following manner, and yield the triatomic alcohol:



The result, however, did not correspond to this anticipation: the only products of the reaction were formiate and acetate of ethyle, acetic acid, and water; and it appears that the body $\text{C H}^4 \Theta^3$ does not exist in the free state. The action of acetic anhydride on the body gave equally unfavourable results.

The researches of Hofmann and Cahours have led to the conclusion that amidobenzoic acid (benzamic acid) comports itself as a base. Griess and Leibius† have examined it from this point of view, and have found that it combines with cyanogen, as does aniline, but the product of the action has acid instead of basic properties. It has the formula $\text{C}^{18} \text{H}^7 \text{N}^3 \text{O}^4$, and is a combination of one equivalent of benzamic acid with two equivalents of cyanogen. It does not crystallize, it is insoluble in water, and difficultly soluble in alcohol and ether. It forms salts with bases.

The authors have found that a great many amido-acids combine in a similar manner with cyanogen, as do also their ethers.

Mucic acid has a composition analogous to that of citric acid, and both acids are decomposed by fusion with potash in a similar manner. How formerly found that the fermentation of citric

* *Société Chimique de Paris*, April 27.

† *Liebig's Annalen*, March 1860.

acid yielded acetic and butyric acids. Rigault* has recently submitted mucate of lime to fermentation, and has found that the products were exactly the same as with citric acid.

By the action of sodium-alcohol upon iodoform, iodide of methylene is formed. Boutlerow, who examined† this question, has repeated‡ his experiments on a larger scale, and has found that two organic acids are also formed. When the crude product of the action was mixed with water, iodide of methylene was precipitated; and when the supernatant liquor was distilled with excess of tartaric acid, an acid distillate was obtained, which was neutralized with soda and evaporated to dryness. On treating this mass with hydrochloric acid, an oily liquid separated which was submitted to fractional distillation. The first portion boiled at a little over 100° . It was found to consist of acrylic acid. Its silver salt had the characteristic serrated form of acrylate of silver, and was reduced on boiling. The analytical results confirmed this conclusion.

The other portion of the distillate boiled between 195° and 198° , but was decomposed by further rectification. It is a colourless, thickish fluid, with a taste suggestive of both acetic acid and *Pelargonium zonale*. It decomposes carbonates, and forms with the alkalis and alkaline earths soluble crystalline salts. It has the formula $C^5 H^{10} O^3$. Boutlerow considers it to be analogous to Heintz's new acid§, and calls it *valerolactic acid*. The small quantity formed has, however, prevented the determination of this point.

Gerhardt expressed the opinion that furfural was the aldehyde of pyromucic acid. Schwanert and Schulz have recently found|| that this hypothesis is correct. According to Schulz, furfural by treatment with oxide of silver is converted into pyromucate of silver, with the separation of metallic silver. Schwanert has found, on the other hand, that freshly prepared furfural forms a crystalline compound with alkaline bisulphites when it is mixed with a concentrated solution and left to spontaneous evaporation over sulphuric acid.

By the destructive distillation of mucate of ammonium, Malaguti obtained a body of the formula $C^5 H^6 N^2 O$, which he called pyromucamide. Schwanert has investigated this body, and considers it to be the amide of a new acid, *carbopyrrolic acid*.

* *Comptes Rendus*, April 1860.

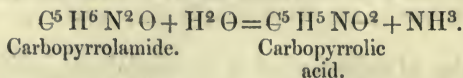
† *Phil. Mag.* vol. xviii. p. 287.

‡ *Liebig's Annalen*, May 1860.

§ *Phil. Mag.* vol. xix. p. 385.

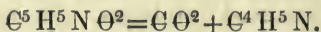
|| *Liebig's Annalen*, April 1860.

When the above body is heated in a sealed tube with baryta water, it is converted into ammonia and the acid:

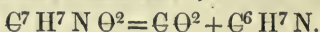


The product of the action is carbopyrrolate of barium, which crystallizes in nacreous laminae. It can be boiled with potash without decomposition, but when treated with hydrochloric acid, a white crystalline precipitate of carbopyrrolic acid is formed.

When the free acid is heated at 60° an atom of carbonic acid is liberated, and the brown flocculent substance is separated which Anderson calls *pyrrol-red*. The decomposition is as follows:



This action is quite analogous to the decomposition of anthranilic acid by destructive distillation,



Kolbe and Lautemann have published a memoir on the constitution of salicylic acid*. They consider that this acid is analogous to lactic acid, and monobasic. For the development of their reasons, which by no means settle the question, the original memoir must be consulted; we shall here merely describe certain new bodies which they have discovered in the course of their investigation.

The starting-point for their research was salicylic acid. This acid has the formula $\text{C}^{14} \text{H}^6 \text{O}^6$, and is isomeric with oxybenzoic acid. The authors consider both these acids as having the same rational formula, which they write $\text{HO}, \left\{ \begin{array}{c} \text{C}^{12} \text{H}^4 \\ \text{HO}^2 \end{array} \right\} \text{C}^2 \text{O}^2, \text{O}$, but as containing two different radicals. Benzoic alcohol and cresylic alcohol have the same composition, $\text{C}^{14} \text{H}^8 \text{O}^2$, and doubtless the same constitution, but they differ in containing different radicals. In like manner, in salicylic acid the radical is phenyle, $\text{C}^{12} \text{H}^5$, while in oxybenzoic acid there is a radical of an alcohol next lower in the series than benzoic alcohol.

When one part of salicylic acid is mixed with three parts of pentachloride of phosphorus, a violent action is set up, torrents of hydrochloric acid are evolved, and a colourless liquid distils over, which consists principally of the chloride $\text{C}^{14} \text{H}^4 \text{O}^2 \text{Cl}^2$, and of oxychloride of phosphorus. Besides these there is some chloride of salicylc, $\text{C}^{14} \text{H}^5 \text{O}^4 \text{Cl}$, and an oil which is unacted on by water.

The chloride, $\text{C}^{14} \text{H}^4 \text{O}^2 \text{Cl}^2$, is obtained in larger quantity by

* Liebig's *Annalen*, August 1860.

treating salicylate of soda with pentachloride of phosphorus. The authors name it *chloride of chlorsalyle*. When it is treated with water, an acid is obtained which has the composition of chlorobenzoic acid, $C^{14}H^5ClO^4$, and was formerly thought to be that acid until Limpricht and Uslar showed that this was not the case. Kolbe and Lautemann name the acid *chlorsalylic acid*. It differs from chlorobenzoic acid in having a different crystalline form, and in being about three times as soluble in water as that acid. The two acids also differ in their melting-point. Unlike salicylic acid, chlorsalylic acid gives no purple colour with perchloride of iron, but, like chlorobenzoic acid, a yellow precipitate. The acids likewise differ in their deportment towards sodium-amalgam: chlorobenzoic acid is attacked with great difficulty, but chlorsalylic acid is readily converted into a new acid.

To obtain this acid, a quantity of sodium-amalgam was added to a concentrated aqueous solution of chlorsalylic acid, and the mixture ultimately heated to boiling for some time; the mercury having been separated, the filtrate was treated with hydrochloric acid, by which a crystalline magma was separated. This, on appropriate purification, was obtained in beautiful crystalline needles.

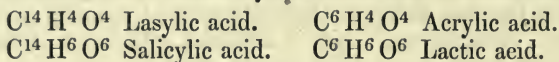
This new acid the authors name *salylic acid*: it has the formula $C^{14}H^6O^4$. It is isomeric with benzoic acid, and stands to salicylic acid in the same relation as propionic acid does to lactic acid*. It crystallizes in small, microscopic needles, the form of which differs from that of benzoic acid. It is about three times as soluble as benzoic acid; it is more volatile, and, unlike that acid, it melts in hot water to a clear oil before it dissolves. The salts, of which many were examined, also differ considerably from those of benzoic acid.

In these investigations there was frequent occasion to determine the solubility of the different acids. This was effected by preparing a solution of the acid of such a strength that, on cooling, very little crystallized out. This was cooled to 0° for several hours and filtered. A measured quantity was then determined by means of a standard alkaline solution.

Chloride of Salyletrichloride, $C^{14}H^4Cl^4$.—This is the oil left after the crude product of the action of pentachloride of phosphorus on salicylic acid has been treated with water. It forms a yellowish oil, which, when dried over chloride of calcium, distils over colourless, and solidifies to a brilliant mass of crystals. Its specific gravity is 1.51; it melts at 30° , and boils at 265° . It has a strong tendency to crystallize. Heated in a closed tube with water, it is converted into hydrochloric and chlorsalylic acids.

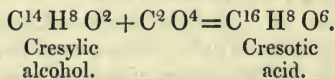
* Phil. Mag. vol. xix. p. 384.

When salicylate of soda is treated with excess of pentachloride of phosphorus, a syrupy fuming liquid distils over, which, on cooling, deposits tabular crystals of a body free from chlorine. The mother-liquor smells of phenylic alcohol. The body is soluble in ether, and on evaporation of the ethereal solution, remains as a white woolly mass of crystals. This body has the formula $C^{26}H^8O^4$; it appears to be a compound of oxide of phenyle with an acid which differs from salicylic acid by containing two atoms of water less. This acid, $C^{14}H^4O^4$, the authors name provisionally *lasylic acid*; it would stand to salicylic acid in the same relation that acrylic acid does to lactic acid:

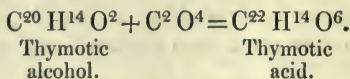


It has been already stated* that these authors obtained salicylic acid by the action of carbonic acid on sodium-phenylic alcohol. They then regarded this acid as being analogous to carbovinic acid. This view they have given up. Such an acid is formed; but as carbovinic acid is decomposed by hydrochloric acid into carbonic acid and alcohol, so the acid is decomposed into phenylic alcohol and carbonic acid; while salicylic acid, which is formed at the same time, is unaltered by it.

When a stream of dry carbonic acid is passed into gently heated cresylic alcohol, $C^{14}H^8O^2$, and sodium added from time to time, a mass is obtained from which, when dissolved in water and treated with hydrochloric acid, a new acid is precipitated crystallizing in beautiful large prisms. It is homologous with salicylic acid, and greatly resembles it. It gives with iron the beautiful purple colour, and when heated it decomposes into carbonic acid and cresylic alcohol. Although it has a higher atomic weight, it melts at 153° , which is 6° below the melting-point of salicylic acid. The formation is thus:—



In like manner, by treating the homologous hydrated oxide of thymyle, $C^{20}H^{12}O^2$, with carbonic acid and sodium under the same circumstances, a new acid is obtained, *thymotic acid*, homologous with salicylic and cresotic acids, and with similar properties. Although it contains 22 equivalents of carbon, its melting-point, 120° , is lower than either of the foregoing acids. Its formation is quite similar.



* Phil. Mag. vol. xix. p. 213.

In the course of these investigations the authors were led to examine* the acids contained in gum benzoin. They found that benzoic acid is not contained in all varieties of the resin. In some cases, especially in the benzoin from Sumatra, an acid was found which crystallized differently from benzoic acid, and melted under hot water to a clear oil, yet by oxidation yielded oil of bitter almonds. This acid they believe to be identical with the toluylie acid obtained by Strecker from vulpic acid†.

LIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 326.]

February 23, 1860.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

THE following communications were read:—

“On the Lines of the Solar Spectrum.” By Sir David Brewster, K.H., D.C.L., F.R.S., and Dr. J. H. Gladstone, F.R.S.

In a paper in the Transactions of the Royal Society of Edinburgh for 1833, Sir David Brewster stated that he had examined the lines of the solar spectrum, and those produced by the intervention of nitrous acid gas, and had delineated them on a scale four times greater, and in some parts twelve times greater than that employed in the beautiful map of Fraunhofer. None of these drawings, however, were published at the time; they were increased by frequent observations continued through succeeding years; and now having been collated, arranged, and added to by Dr. Gladstone, they form the diagrams accompanying this paper.

The figures consist of—

1st. A map of the whole spectrum 58 inches long, and exhibiting about 1000 lines and bands. This map includes a great prolongation of the spectrum at the least refrangible end, before A, with a series of bands and lines not hitherto described.

2nd, 3rd, 4th, and 5th. Enlarged delineations of the portions of the spectrum between A and B, and between E and F, exhibiting additional lines, with still more magnified views of the groups *a* and *b*.

6th. A map of the two extremities of the solar spectrum as observed by Dr. Gladstone about noon-day at midsummer, consequently when the sun was at its greatest altitude. This shows several bands between A and B, and a series of lines in the lavender rays extending as far as M. Becquerel's N, and corresponding evidently with the maps published by him and by Professor Stokes, with the addition of finer lines. Yet this map represents the extreme spaces of the spectrum where there is no effect on the organ of vision, while that of M. Becquerel represents the want of chemical action, and that of Professor Stokes the absence of fluorescent power.

* Liebig's *Annalen*, July 1860.

† Phil. Mag. vol. xix. p. 211.

7th. A map of the "atmospheric lines" compiled from the independent observations of the two authors. These lines and bands are visible only when the sun is rising or setting, that is to say, when his beams traverse a long stratum of our atmosphere. In some cases they are merely the deepening of bands seen at any time, in other cases they are bands which appear for the first time when the sun is close to the horizon. Professor Piazzi Smyth has represented some of these lines in his delineations of the spectrum as observed from the Peak of Teneriffe, whence he had the advantage of seeing the sun at an altitude of $-1^{\circ}.1$. The most remarkable of the atmospheric lines are situated in the orange and yellow spaces, and one band just beyond D is discernible in the diffused light of a dull day at any hour, though it covers what is about the brightest part of the prismatic image obtained from direct sunshine. The western sky after sunset exhibits these phenomena in a striking manner, and with some variations that do not appear to depend altogether on the absence or presence of moisture, although when the sun looms red through a fog these lines also make their appearance. They are in no respect due to the mere reduction in the quantity of light.

The dispersion and absorption of the more refrangible rays by the atmosphere, and by fogs, smoke, and such media as dilute milk and water, is a quite independent phenomenon.

8th and 9th. Enlarged views of A and B, when the light is acted upon by a long passage through the atmosphere.

10th. A map of the spectrum, exhibiting on a large scale the dark lines and bands which were seen by Sir David Brewster when nitrous acid gas is interposed between the prism and the source of light. Their position is identified by the insertion of the principal lines of the solar spectrum. They differ considerably from a smaller drawing of the same by Professor W. A. Miller, who employed a deeper stratum of the red gas.

The light of the moon, which is only that of the sun reflected from her surface, exhibits all the principal lines from about B to H, and no fresh ones; and when the luminary was near the horizon, the more prominent atmospheric lines were detected. The green colour was observed to extend a little beyond F in the spectrum of moonlight, and the space between G and H appeared lavender or lavender-grey instead of violet.

In respect to the origin of these lines, it is conceivable that the light when emitted from the photosphere itself is deficient in these rays, or that they are due to absorption by the atmosphere of the sun, or by that of the earth. The first of these suppositions scarcely admits of a positive proof. If the second be true, it might be expected that the light from the edge of the solar disk would exhibit more of these absorption bands than that from the centre, which must have traversed a smaller amount of atmosphere; but such was not found to be the case. The third supposition is favoured by the fact that the atmosphere has unquestionably much to do with the manifestation of many of these lines, and by the analogy of the bands produced by nitrous acid gas, bromine vapour, and other absorbent media. The experi-

mentum crucis of observing an artificial light through a long space of air was attempted by means of the revolving light on Beachy Head, as seen from Worthing at a distance of twenty-seven miles. It gave a negative result; but on account of the great difficulty of detecting slight breaks in a faint thread of light, no great reliance is to be placed on the experiment. A similar doubt rests on the authors' observations of fixed stars, and on the non-recognition by Fraunhofer of the ordinary lines in the light of Sirius and Castor, while on the other hand he did detect D and *b* in that of other stars. The origin of these lines is still an open question.

The spectra of artificial flames sometimes exhibit bright lines coincident with the dark spaces of the solar spectrum. Thus the yellow band in the flames of soda, and several other substances, is identical in refrangibility with D; but the most remarkable case is that of charcoal or sulphur burnt in nitre; the spectrum shows three very prominent lines, two of which coincide with A and D, while a faint red line appears at B, and a group between it and A.

A map is also given of the bright lines, principally orange, that make their appearance when nitrate of strontia is placed on the ignited wick of a spirit-lamp.

“On some New Volatile Alkaloids given off during Putrefaction,”
By F. Crace Calvert, Ph.D., F.R.S. &c.

Some eighteen months ago my friend Mr. J. A Ransome, surgeon to the Royal Infirmary, Manchester, induced me to make some researches with the view of ascertaining the nature of the products given off from putrid wounds, and more especially in the hope of throwing some light upon the contagion known as hospital gangrene. I fitted up some apparatus to condense the noxious products from such wounds; but the quantity obtained was so small, that it was necessary for me to acquire a more general knowledge of the various substances produced during the putrefaction of animal matter, before I could determine the nature of the products from sloughing wounds. I therefore began a series of experiments, the general results of which I now wish to lay before the Society.

Into each of a number of small barrels twenty lbs. of meat and fish were introduced, and to prevent the clotting together of the mass, it was mixed layer by layer with pumice-stone. The top of each barrel was perforated in two places, one hole being for the purpose of admitting air, whilst through the other a tube was passed which reached to the bottom of the barrel. This tube was put in connexion with two bottles containing chloride of platinum, and these in their turn connected with an aspirator. By this arrangement air was made to circulate through the casks, so as to become charged with the products of putrefaction and to convey them to the platinum salt. A yellow amorphous precipitate soon appeared, which was collected, washed with water and alcohol, and dried. This precipitate was found to contain C, H, and N, but what is highly remarkable, sulphur and phosphorus enter into its composition. The presence of C, H, and N was ascertained by elementary analysis; for the sulphur and

phosphorus, a given weight of the platinum salt, 0.547 grm., was oxidized with nitric acid, and gave 0.458 grm. of sulphate of baryta = 11 per cent. of sulphur, and 0.266 of pyrophosphate of magnesia = 6.01 per cent. of phosphorus. I also ascertained the presence of these two substances by heating a certain quantity of the platinum salt with strong caustic ley, when a liquid, volatile and inflammable alkaloid was obtained, whilst the sulphur* and phosphorus remained combined with the alkali and were easily detected. I satisfied myself during these researches, which have lasted more than twelve months, that no sulphuretted nor phosphuretted hydrogen was given off; and my researches, as far as they have proceeded, tend to prove that the noxious vapours given off during putrefaction, contain the N, S, and Ph of the animal substance, and that these elements are not liberated in the simple form of ammonia, and sulphuretted and phosphuretted hydrogen. I also remarked during this investigation, that, as putrefaction proceeds, different volatile bodies are given off.

Before concluding, I may add, that when the platinum salts are heated in small test-tubes, they give off vapours, some acid and some alkaline, possessing a most obnoxious and sickening odour, very like the odours of putrefaction; and that at the same time a white crystalline sublimate, which is not chloride of ammonium, is formed.

March 1, 1860.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communications were read:—

“On the Electrical Phenomena which accompany Muscular Contraction.” By Professor C. Matteucci.

Dr. Radcliffe has recently communicated to the Royal Society some observations on the nature of the electrical phenomena accompanying muscular contraction. It is known that M. du Bois-Reymond admits that the muscular current diminishes during contraction, and that he attributes the phenomena indicated by the galvanometer to the momentary predominance of currents due to the polarization of the electrodes of platinum over the muscular current. In my last memoir on Electro-physiology, which was communicated to the Royal Society and appeared in the Philosophical Transactions for 1856, I proved that these phenomena take place independently of the existence of secondary currents of the electrodes, and I hence concluded, at least as regards the muscles of frogs, that during contraction there is a current, or rather an instantaneous electrical discharge, which takes a contrary direction to that of the *relaxed gastrocnemius*, and in general to that of the current which is found on applying the extremities of the galvanometer to the extremities of the limbs of a frog.

In order to avoid the influence of secondary polarity, M. du Bois-Reymond, and after him several other German physiologists, have

* Some of the platinum salt was treated with $C S_2$, which did not remove any free S, and the beautiful orange-yellow colour of the precipitate showed the absence of sulphuret of platinum.

thought it expedient to contract and tetanize the gastrocnemius before closing the circuit of the galvanometer; the deviation thus obtained is feebler than that which is due to the current of a muscle in repose, but never in a contrary direction to that due to this current.

I have already remarked* that this result accords with that which is obtained by the ordinary experiment, in which the muscular current is in circulation previously to the contraction of the muscle. In fact, we know that by continuing to keep the muscle in contraction, above all when the muscle remains tetanized, the electric phenomenon accompanying contraction (the effect of which is to produce a deviation of the needle in a contrary direction to that of the current of a relaxed muscle) becomes gradually less intense as the contractions are more and more feeble.

The method employed by Dr. Radcliffe is the same as that which I followed in my latest experiments; that is, he made use of amalgamized plates of pure zinc as electrodes, immersed in a neutral solution of sulphate of zinc, and after having ascertained that there was nothing to fear from the effects of secondary polarity, he says that he finds that the needle deviated by the muscular current descends, during contraction, towards zero, but *only more slowly* than it would have done had the circuit been opened.

Dr. Radcliffe next examines another of my experiments, in which, instead of placing a *gastrocnemius* in the circuit, I employ a thigh cut transversely at the upper extremity, so that the needle remains deviated in a contrary direction to that of the gastrocnemius. In this arrangement of the experiment, when contraction is produced, the deviation of the needle increases, which is perfectly in accordance with the idea that during contraction a muscular current is developed in a contrary direction to the current of the relaxed gastrocnemius. Dr. Radcliffe attempts to explain this result by supposing (if I rightly understand his idea) that during contraction the contacts with the electrodes are deranged so as to facilitate the passage of the current of the relaxed muscle.

Being unwilling to remain in doubt as to the nature of the electrical phenomena of muscular contraction, I have of late repeated and varied my experiments.

As to Dr. Radcliffe's first remark, I shall only observe that in the principal experiment the needle does not merely move slowly towards zero during contraction, but is seen, during the first contractions, especially when the frog operated on is vivacious, to move rapidly down to zero, to oscillate, to pass to the opposite side, and sometimes even to remain fixed, while thus deviated, for a very short interval of time. This result, which is easily obtained and can be verified without difficulty, is the same, whether the electrodes are of platinum, like those employed by M. du Bois-Reymond, or of zinc.

It is easy to understand that, in order to succeed in these experiments, it is desirable that the needle should be as little deviated as

* *Nuovo Cimento*, September 1858, p. 238.

possible before the contractions: this object is best ensured in the following way:—I prepare the frog by reducing it to two thighs, leaving a single lumbar nerve in order to obtain contractions in one of the thighs. Instead of saturated solution of sulphate of zinc, I employed a weak solution of this salt, in order to avoid any alteration of the surface of the muscles; and finally, in order to maintain exactly the same points of contact between the two electrodes and the two near points of the middle portion of the thigh, I employ two fine woollen cords or two thin strips of card-board fixed with sealing-wax on a plate of glass and soaked in the same solution. The experiment is made by applying the glass plate with a certain pressure on the thigh, so that the two cords on one side touch the thigh, and on the other are placed in contact with the cushions of flannel or card-board which are immersed together with the electrodes, according to the method followed by M. du Bois-Reymond.

I think it useful to describe in a few words a little apparatus which affords a good deal of facility for making these experiments. It consists in a small square block of wood, with a cavity deep enough to receive the electrodes and the cushions. It is hardly necessary to say that this cavity is coated with a varnish of sealing-wax and divided in the middle by a glass plate. Another cavity in the same block serves as a recipient for the two thighs; the sciatic nerve extends beyond the block, and rests on two platinum wires which communicate with the pile or with the electro-magnetic machine. The communication between the thigh and the electrodes is established by means of the glass plate in the manner above described, that is, I press this strip of glass slightly on the middle of the thigh on one side, and at the same time the extremities of the two woollen cords come to rest on the cushions. The movements of the needle are observed through a telescope (lunette). I have repeated this experiment thirty or forty times. Sometimes, and this case is the most frequent, the first deviation produced by the muscle in repose is directed in the same sense as that of the current of the gastrocnemius; sometimes the current is null, or almost null; sometimes, and this case is the most rare, the deviation is in a contrary direction, and this occurs most frequently in operating on the hinder portion of the thigh.

In all these experiments, the moment that the thigh begins to contract, the needle moves in a constant direction; the deviation which intervenes is greater or less according to the force of the contraction, and indicates constantly a descending discharge or current of extremely short duration, which traverses the thigh in the direction of the ramification of the nerves, and in a contrary direction to the current of the gastrocnemius.

“An Inquiry into the Muscular Movements resulting from the action of a Galvanic Current upon Nerve.” By Charles Bland Radcliffe, M.D., F.R.C.P.

In a lecture delivered about two years ago*, in which he treats

* *Leçons sur la Physiologie et Pathologie du système nerveux. Tome I. Leçon 10. Paris, 1858.*

among other things of the muscular movements resulting from the action of a galvanic current upon a motor or mixed nerve, Professor Claude Bernard says that some of the more important of these movements have been overlooked, and he quotes an account of some investigations by Dr. Rousseau of Vezy, which do away with certain very perplexing variations in the order of these movements.

The movements resulting from the action of a galvanic current upon nerve are usually divided into the three periods of double, alternate, and single contraction which are set down in the following Table:—

TABLE I.—*The Nerve divided and lifted up at its end.*

	Direct Current.		Inverse Current.	
	Beginning.	End.	Beginning.	End.
Period of double contraction . .	Strong contraction.	Contraction.	Contraction.	Contraction.
Period of alternate contraction . .	Contraction.	0	0	Contraction.
Period of single contraction . .	Contraction.	0	0	0
Apparent irregularity—"Voltaic alternatives."				

Professor Bernard proposes to place another period before the first of these—a period corresponding to the normal unexhausted and undisturbed state of nerve, and characterized by contraction at the beginning of the two currents, direct as well as inverse.

The investigations of Dr. Rousseau show how it is that the order of these contractions is altered by certain changes in the arrangement of the nerve acted upon by the current. If *the nerve acted upon be divided and lifted up at its end*, so as to break the circuit of the *derived current**, the order of contraction is that which is put down in the preceding Table; if *the nerve acted upon be raised in a loop* (either without dividing it, or, after dividing it, by dropping down the end), so as not to break the circuit of the derived current, the order of contraction is that which is represented in the following Table:—

* Figures 3 & 4 on page 396 may serve to illustrate all that need be said respecting the *derived current*. In figure 3, the sciatic nerve of a frog's leg is arranged in a loop across the poles P N of a galvanic apparatus; the *primitive current*, indicated by the black arrow, passes by the shortest route from the positive pole P to the negative pole N; the *derived current*, indicated by the dotted arrows, passes by the longest route between these poles, and as it also proceeds from the positive pole P to the negative pole N, it passes in the contrary direction to that of the primitive current. In fig. 4, the sciatic nerve is divided between P and the thigh, as is meant where the nerve is spoken of as *divided and lifted up at its end*; and being divided, the only current passing is the primitive current; for the circuit of the derived current being broken, there can be no derived current in this case.

TABLE II.—*The Nerve in a loop.*

	Direct Current.		Inverse Current.	
	Beginning.	End.	Beginning.	End.
Period of double contraction .	Strong contraction.	Contraction.	Strong contraction.	Contraction.
	Contraction.	Contraction.	Strong contraction.	Contraction.
Period of alternate contraction . .	0	Contraction.	Contraction.	0
Period of single contraction . .	0	0	Contraction.	0
Apparent irregularities—"Voltaic alternatives."				

Now Dr. Rousseau shows that the order of contraction set down in the second Table is due to the action of this *derived current*. He shows, also, that by excluding the derived current (which he does by means of an ingenious triple arrangement of poles called the "rhéophore bifurqué"), the order of contraction becomes one and the same in the case where the nerve is divided and lifted up at its end, and in the case where the nerve is arranged in a loop, the order being that which is set down in the first Table.

On inquiring into these matters experimentally, the author finds reason to believe that Professor Bernard has wandered into some degree of error, and that the path of inquiry opened out by Dr. Rousseau is not only a right path, so far as its discoverer has traced it out, but that it leads to the explanation of some very curious alternating movements which have not hitherto been described. He has been led, also, to form certain conjectures respecting the *modus operandi* of electricity in muscular motion which he trusts may serve to simplify this very difficult and complex subject.

1. When nerve is in its normal, unexhausted, undivided state, there is, as Professor Bernard points out, contraction at the beginning of both currents, inverse as well as direct, and at the beginning only, *if a feeble current be used*. This, for example, will be the result of the application of the feeble current produced by partially moistening the small galvanic forceps of Bernard with saliva. But it is also a fact, that a stronger current—the current for example of a Pulvermacher's chain of ordinary length moistened with vinegar—will produce contraction at the end as well as at the beginning of both currents (as in the period of double contraction), if applied to a nerve similarly circumstanced. Nay, it is a fact, that the feeble current of the forceps will give contraction at the beginning of both currents, and at this time only, *after* the stronger current of the chain has produced contractions at the end as well as at the beginning of both currents, and that we may produce alternately again and again the contraction confined to the beginning, and a contrac-

tion occurring at the end as well as at the beginning, of the currents, by applying alternately the feeble current of the forceps and the stronger current of the chain. The author finds, also, that the feeble current of the forceps will produce contraction at its end as well as at its beginning, if the nerve be raised and placed as a loop across the points of the forceps; and not only so, but that the same current will produce contraction only at its beginning, if it be applied after slipping away the points of the forceps, and so allowing the nerve to fall back upon the muscles. Hence the single contraction at the beginning of a *feeble* inverse or direct current, and not at the end, instead of indicating, as Professor Bernard supposes, the normal state of undisturbed and unexhausted irritability in the nerve, must only be looked upon as the result of the action of a feeble current under particular circumstances. In a word, the fact is one which reflects the strength of the current rather than the condition of the nerve.

2. The curious alternating movements, which do not appear to have been described hitherto, and which may be explained by means of the key which Dr. Rousseau has put into our hands, are best seen when the current is made to act upon the lumbar nerves of one side, but they are also seen in the case where a loop of sciatic nerve is acted upon.

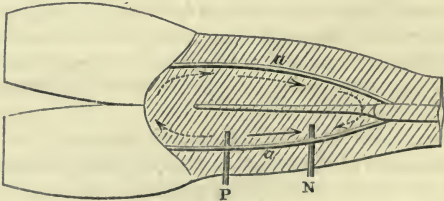
Take the back, loins, and hind limbs of a frog with the lumbar nerves properly exposed, raise the nerves on one side into a loop without dividing them, place them over the platinum poles of a galvanic apparatus (a Pulvermacher's chain of ordinary length), and pass the current. On doing this, as might be expected, there is in the first instance, contraction in the limb to which the nerves acted upon belong, *but this contraction is slight and transient when compared with the contraction which is set up in the opposite limb, the nerves of which are not acted upon.* In this opposite limb, indeed, the contraction is sure to be both strong and tetanic. A little later (and it is to the phenomena of this stage that the author wishes to direct attention), and the results are as follows:—With the *inverse primitive current* there is contraction in the leg belonging to the same side when the current begins to pass, and contraction in the leg belonging to the opposite side when it ceases to pass; with the *direct primitive current* this order of contraction in the two legs is reversed.

In bringing about these curious alternations, the action of a *derived current* is obviously concerned; for on excluding this current by means of Dr. Rousseau's rhéophore bifurqué, they come to an end, and the movements resulting from the action of the current are confined to the leg, the nerves of which are directly acted upon. It is evident, also, that a derived current is what is wanting to produce the contraction in the limb belonging to the opposite side; for after breaking the circuit of the derived current by dividing the lumbar nerves where they emerge from the spine, and separating the divided ends, and after then completing the circuit by dropping down the end of the divided nerve, or by bridging over the gap by

a piece of wet string or paper, by a strip of the animal's skin, by a piece of wire, or by any other conductor, it matters not what, the contractions occur alternately in the two legs just as they did before the nerve was divided. Nay, it may be argued from the following experiment, that reflex nervous action has nothing to do in producing these alternations. Divide the lumbar nerves on one side, not where they emerge from the spine, but where they pass into the thigh; raise the divided end of the nerve, and place it across the poles of the galvanic apparatus. In this case the circuit of the derived current is broken, and the action of this current is therefore put out of the question. In this case, the nerve acted upon by the current is still in connexion with the spinal cord, and through the cord and the nerves proceeding from this cord, with the limb on the opposite side; and hence it might be supposed that the current might irritate the cord, and so provoke contraction in the limb on the opposite side. But the simple fact is, that the current may be passed inversely or directly without producing contraction anywhere, except now and then a few flickers in the muscular fibres in the lumbar region of the side corresponding to that of the nerve operated upon. The simple fact, indeed, appears to show that reflex nervous action can have nothing to do with the contractions in the limb belonging to the opposite side, which contractions are produced by the action of the galvanic current on one set of lumbar nerves; and, certainly, with the key furnished by Dr. Rousseau, reflex nervous action is not required to explain the phenomenon.

The following diagram will give the case in which the lumbar nerves on one side are acted upon by the *inverse primitive current*, *a a* being the lumbar nerves, P N the poles of the galvanic apparatus, the black arrow the primitive current, the dotted arrows the derived current. The results, as seen in contraction in the limb belonging to the same side, or in that belonging to the opposite side, are seen in the Table below the figure. The case is plain.

Fig. 1.

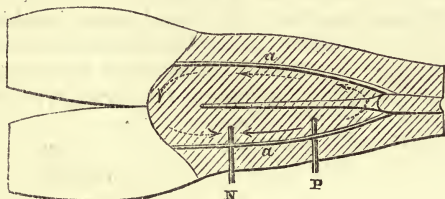


	The Inverse Current.	
	Beginning.	End.
On the same side . . .	Contraction.	0
On the opposite side . .	0	Contraction.

On the side acted upon by the inverse primitive current, the portion of nerve nearest to the muscles supplied by the nerve (the muscles of the leg) is traversed, not by the inverse primitive current, but by a *direct* derived current; and hence we should expect to find in the leg on this side (for at the time of these alternate contractions the nerve is in the state in which the current produces contraction alternately at the beginning of the direct current and at the end of the inverse current) the effects of a direct current—contraction at the beginning of the current. We should expect to find this; for of two currents acting upon the same nerve, it is the one nearest to the muscles supplied by the nerve which acts upon these muscles. In the limb on the opposite side we should expect, on the contrary, the effects of an inverse current—contraction at the end of the current, for the lumbar nerves on this side are traversed by an *inverse* derived current; and this, as the Table shows, is actually the case.

A similar diagram and table will show that the results of passing a *direct primitive current* through a portion of the lumbar nerves on one side are in accordance with the same law. In this case, as in the other, the acting current on both sides is the derived current. On the side acted upon by the direct primitive current, the acting derived current (acting because nearest to the muscles supplied by the nerve) is *inverse*; and therefore the limb on this side ought to contract at the end of the current. On the opposite side, the course of the derived current is *direct*, and therefore the limb on that side ought to contract when the current begins to pass: and so it is.

Fig. 2.

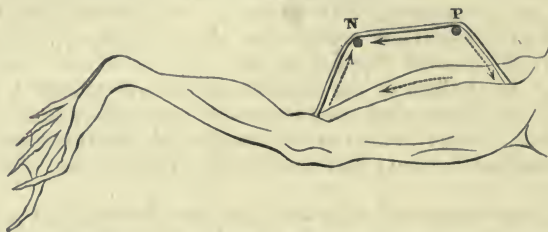


	The Direct Current.	
	Beginning.	End.
On the same side . . .	0	Contraction.
On the opposite side . .	Contraction.	0

The results of the action of a galvanic current upon a loop of sciatic nerve are, after a time, analogous to those which have just been mentioned. At first, the contraction attending upon the beginning and ending of both currents affects the whole limb; after a time, the leg and thigh contract alternately, in an order which changes with the direction of the current.

Let the following diagram and table represent the case in which a loop of sciatic nerve is acted upon by the *direct primitive current*, α being the nerve, P N the poles of the galvanic apparatus, the black arrow the primitive current, the dotted arrows the derived current ; and it will be seen that the portion of nerve between the negative pole and the leg is acted upon by an *inverse* derived current, and that the thigh is traversed by a *direct* derived current. Thus—

Fig. 3.

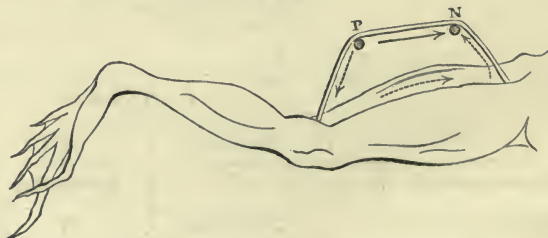


	The Direct Current.	
	Beginning.	End.
Thigh	Contraction.	0
Leg	0	Contraction.

Hence there ought to be, as there is in fact, and as the Table shows, contraction in the thigh when the current begins to pass, and in the leg when the current ceases to pass.

A similar diagram and table will give the case in which a loop of sciatic nerve is acted upon by the *inverse primitive current*, and show at a glance that the leg ought to contract at the beginning of the current, because the current, acting upon the portion of nerve nearest

Fig. 4.



	The Inverse Current.	
	Beginning.	End.
Thigh	0	0
Leg	Contraction.	0

to the leg, is *direct* derived current. The diagram will also seem to show that the thigh ought to contract at the end of the current, for the thigh is traversed by *inverse* derived current. In fact, however, the thigh does not contract either at the beginning or at the end of the current; and this perhaps is not to be wondered at; for the author finds that contraction attends upon the beginning of a *direct* current of a given strength for some time after it has ceased to attend upon the end of an *inverse* current of the same strength.

3. The *modus operandi* of galvanism upon a motor or mixed nerve is a subject beset with difficulties; but some of these difficulties do not appear to be altogether insurmountable.

(a) In looking at the movements belonging to the first period—that of *double contraction* (*vide* Table I.)—it is not difficult to find a reason which will in some degree explain how it is that contraction is confined to the beginning and end of the current. It is not difficult to see that the beginning and ending of the galvanic current in the nerve may involve certain changes in the strength of the nerve-current, and that these changes may in their turn give rise to momentary induced currents in the nerve and in the neighbourhood; for such momentary currents are induced, not only when a current begins to pass and when it ceases to pass, but also at the moments when it undergoes any change of strength. It is not difficult to see, also, that the muscular fibres to which the nerve is distributed may be the seat of some of the secondary currents thus induced, and that these fibres may be thrown into contraction by these currents. Nor is it difficult to see—if the contraction be thus connected with the induced currents—that there will be no contraction in the interval between the beginning and ending of the inducing galvanic current; for if the inducing current exhibits no variation in strength, there is no secondary current induced in this interval.

(b) It is, perhaps, too much to expect at present a full explanation of *the second period of contraction*—of that period, that is, in which the contraction alternates at the beginning of the direct and at the end of the inverse current; but the author is disposed to think that a partial answer may be found in the collation of the three facts which follow.

The first fact is this—that the direction of the nerve-current in the sciatic and lumbar nerves of a frog (except in those last moments of all in which the action of the galvanic current upon the nerve gives rise to the “voltaic alternatives”) is *inverse*. In these last moments the nerve-current in these nerves may be sometimes direct, some-

Fig. 5.



times inverse, and this change of direction may take place more than once; but except in these last moments, the author finds the direction of the nerve-current to be invariably inverse.

The second fact is furnished by Professor du Bois-Reymond in an experiment in which the two ends of a long portion of nerve are placed upon the cushions of two galvanometers, and the middle of the nerve is laid across the poles of a galvanic apparatus. Looking at the needles of the galvanometers before passing the galvanic current, they are seen to diverge under the action of the nerve-current; and from the direction of the divergence, it is evident that this current passes from the end to the side of the nerve. Looking at the needles while the galvanic current is passing, one needle is found to move still further from zero, the other is found to return towards zero. Let AB be the nerve; let the arrows aa' and bb' be the nerve-currents included between the cushions aa' and bb' of two galvanometers; and let the arrow PN be the current between the poles PN of the galvanic apparatus; and under this arrangement the needle of the galvanometer will recede, and show increase of cur-

Fig. 6.



rent ($+$) at the end B , where the nerve-current and galvanic current coincide in their direction; and at the end A , where the two currents, natural and artificial, do not coincide in their direction, the needle of the galvanometer will go back, and show decrease of nerve-current ($-$).

The third fact, which has been recently furnished by Professor Eckardt, is to be found in an experiment which may be illustrated by means of the two following figures. In this experiment, the nerve of a properly prepared frog's leg is placed, one portion (that nearest to the leg) across the poles $I I'$ of an induction coil, another portion across the poles PN of a galvanic apparatus. Having done this, the leg is first thrown into a state of tetanus by passing a series of induced currents, and then (the tetanizing influence still continuing in operation) the continuous current of the galvanic apparatus is transmitted from P to N . This is the experiment. The result is that the tetanus ceases when, as in fig. 7, the inverse current

Fig. 7.



passes, and continues when, as in fig. 8, the direct current passes.

Fig. 8.



Nor is this result altered by inverting the order in which the con-

tinuous and induced currents are made to act upon the nerve. Thus the induced currents produce contraction if applied after the direct continuous current, but not if applied after the inverse continuous current. Nay, it would even seem as if the direct current is actually favourable to contraction; for a solution of salt, which of itself is too weak to produce tetanus when applied to a nerve, will have this effect when a direct current is made to pass through another portion of the same nerve. In performing this experiment, Professor Eckardt proceeds as follows:—First of all he tetanizes a frog's hinder limb by placing a portion of the nerve nearest to it in a strong solution of salt; after this he adds water until the strength of the saline solution is no longer sufficient to keep up a state of contraction in the muscles; then, all things being as they were, he passes the direct current through a portion of nerve which is not immersed in the solution. *The result is that the tetanus immediately returns.*

Now, on comparing this last fact with the two previous facts, we may have, as it seems to the author, some insight into the mode in which the galvanic current acts upon nerve in the period of alternate contraction. On the one hand, it is seen that tetanus is prevented or arrested by the inverse current; that is to say, tetanus is prevented or arrested when (as the first and second facts show) the galvanic current coincides in direction with, and imparts power to, the nerve-current. On the other hand, it is seen that tetanus is *not* prevented or arrested by the direct current; that is to say, tetanus is *not* prevented or arrested when (as the first and second facts still show) the galvanic current differs in direction from, and diminishes the power of, the nerve-current. The one result is in harmony with the other; for if contraction is counteracted by imparting power to the nerve-current, it is to be expected that contraction will be favoured by detracting power from the nerve-current; and certainly it is no matter of wonder that contraction should be favoured by detracting power from the nerve-current, for it is an established fact that *rigor mortis* is coincident with absolute extinction of the nerve and muscular currents, and that ordinary contraction is attended by unmistakeable *weakening* of these currents. It is also an established fact, that muscular contraction is produced by the *discharge* of ordinary statical electricity, and not by the charging and charge. Nay, it is not improbable that the contractions at the beginning and ending of the current, in the period of double contraction, which contractions have been referred by the author to the action of induced currents, may in reality be due to the *withdrawal* rather than to the *communication* of these currents; for these induced currents are of momentary duration, disappearing at the very instant of appearing, and exhibiting peculiarities in disappearing which connect the disappearance with the *discharge* of statical electricity, rather than with the more quiet cessation of current electricity.

And if this be so—if the inverse current antagonizes and the direct current favours contraction—then we may in some degree understand how it is that contraction occurs alternately at the beginning of the direct, and at the end of the inverse current.

When the *inverse current* passes, there is no contraction at the beginning of the current, for the influence of this current upon the nerve-current is one which antagonizes contraction; when the inverse current ceases to pass there is contraction, for then the influence which antagonized contraction is removed. When, on the other hand, the *direct current* passes, there is contraction at the beginning of the current, for the influence of the current upon the nerve-current is one which favours contraction; when the direct current ceases to pass, there is no contraction, for then the influence is no longer one which favours contraction.

(c) In the third period—*that of single contraction*—the muscular movements resulting from the action of a galvanic current upon nerve are at first sight somewhat perplexing; but with a little thought, it may be seen that the same key will apply to their interpretation.

If, as has just been mentioned, contraction attends upon the beginning of the direct current because this current is found to favour contraction, it is not difficult to find a reason which will explain in some degree, not only why in the *period of double contraction* the strongest contraction is at the beginning of the direct current, but also why in the first part of the period now under consideration—*that of single contraction*—contraction should continue to attend upon the *direct* current after it has ceased to attend upon the *inverse* current. Nor are the apparent irregularities in contraction, the “voltaic alternatives,” entirely inexplicable; for it may be that these seeming irregularities—this apparent shifting of contraction from the beginning of the direct to the beginning of the inverse current, and so backwards and forwards once and again—may be nothing more than the natural consequence of the changes which at this time have taken place, and are taking place, in the direction of the nerve-current.

“Letter from Lord Howard de Walden and Seaford, on a recent severe Thunder-storm in Belgium.”

GEOLOGICAL SOCIETY.

[Continued from p. 245.]

June 13, 1860.—L. Horner, Esq., President, in the Chair.

The following communications were read:—

“On some Arrow-heads and other Instruments found with Horns of *Cervus megaceros* in a Cavern in Languedoc.” By M. E. Lartet, For. M.G.S. (In a letter to the President.)

In a cavern of the limestone at Massat, near Tarascon in Languedoc (Department of the Ariège), examined by M. A. Fontan, the floor was found to consist of a blackish earth, with large rounded pebbles, among which were mixed, in great disorder, bones and horns of a Chamois, *Cervus pseudovirginianus*, *C. megaceros*, and *Bos*, together with implements of stone and bone, to which MM. Isidore Geoffroy Saint-Hilaire and E. Lartet have referred in the ‘Comptes Rendus’ of May 10, 1858.

M. E. Lartet, in his letter, has furnished drawings and descriptions

of some barbed arrow-heads of bone, some having indented grooves, probably for the appliance of poison; also needles, and a flute-bevelled tool of bone, a splinter or knife of hard flint, and the horn of an Antelope hacked at the base, probably when the animal was flayed.

“On the occurrence of Crag Shells beneath the Boulder-clay in Aberdeenshire.” By T. F. Jamieson, Esq.

In a former paper (Q. J. G. S. vol. xiv. pp. 522–525) the author referred to the existence of gravelly beds containing marine shells underlying the boulder-clay between Cruden and Slains, on the coast of Aberdeenshire, over an area of about 6 miles by $3\frac{1}{2}$; these shelly sands and gravels he has since more carefully examined, and he refers them to the age of either the Red or the Mammaliferous Crag of England. *Cyprina rustica*, *C. Islandica*, *Astarte* spp., *Venus* spp., *Artemis linctæ*, *Cardium* spp., *Pecten opercularis*, var. *Audouini*, *P. maximus*?, *P. princeps*?, *Pectunculus glycymeris*, *Tellina solidula*, *Mya truncata*?, *Fusus antiquus* and its var. *contrarius*, *Mangelia*, *Purpura Lapillus*, var. *crispata*, occur in worn fragments. *Cyprina Islandica* is the most abundant.

Chalk-flints are common among the materials of the beds in question; also fragments of fossiliferous limestone and of red and grey sandstones, of undetermined age.

“On some small fossil Vertebræ from near Frome, Somersetshire.” By Prof. Owen, F.R.S., F.G.S.

In this communication Prof. Owen described three minute Vertebræ discovered by Charles Moore, Esq., F.G.S., in an agglomerate occupying a fissure of the Carboniferous Limestone near Frome in Somersetshire, in company with teeth of a small Mammal allied to the *Microlestes* of Plieninger. The vertebræ are stated to correspond in size with the teeth of *Microlestes*; but to have Reptilian characters, especially in their biconcave structure,—a character common in Mesozoic Saurians, but rare in the existing genera. There appears to be but very slight grounds for supposing that such a character may have ever belonged to any Mammals, although some of the existing *Monotremata* have peculiar vertebral modifications somewhat resembling, in these respects, the structural features of Reptiles. In their large and anchylosed neural arch, however, these little vertebræ present a mammalian character.

Remains also of small Saurians and Fishes occur in considerable numbers with the vertebræ in question, as well as the more rare mammalian teeth.

LIV. Intelligence and Miscellaneous Articles.

ON THE LAW OF THE PROPAGATION OF ELECTRICITY IN IMPERFECT CONDUCTORS. BY J. M. GAUGAIN.

THE phenomena of tension, which always accompany the propagation of electricity, manifest themselves in imperfect conductors with an intensity which greatly facilitates their study,—as, for example, when the electricity developed by a machine and accumulated on a conductor passes into the ground through a cotton thread, a

silk ribbon, or a column of oil. On the other hand, no electrodynamic effects are seen under these conditions, and it is not evident *à priori* that the slow movement of electricity is subject to the same laws as that which regulates electric currents. The object of this memoir is to show that Ohm's law, which applies to a constant current, applies also to the slow propagation of electricity in cases in which the electric flow may be considered to be constant. He has found that the mode of propagation is the same in both cases, and that the results attained in studying the case of imperfect conductors, throw great light on important questions in reference to true currents.

The instrument which serves to measure the tension is a gold-leaf electroscope; the separation of the leaves is determined by means of a telescope and a scale. The author employs two different methods for measuring the intensity of the electric flow. The method of the *duration of the flow* consists in giving such a charge to the electroscope that the gold leaves take a position A; it is put in communication with the ground by means of the conductor through which the electricity flows, and the time t is measured which elapses while the gold leaves pass from the position A to another position B. Q being the quantity of electricity lost by the electroscope, $\frac{Q}{t}$ is the electric loss of the electroscope in the unit of time. The conductor is removed and the experiment is repeated, and $\frac{Q}{T}$ is obtained, which is the electricity lost by the electroscope alone in the unit of time. Consequently $Q\left(\frac{1}{t} - \frac{1}{T}\right)$ represents the intensity of the electric flow in the conductor employed. The second method consists in employing a discharging electroscope the gold-leaf of which comes in contact with a small copper ball communicating with the ground. The conductor in which electricity is propagated puts a reservoir of electricity in connexion with this electroscope; and the intensity of the flow may be obtained from the number of its discharges in a given time.

Some of the principal results only can be enumerated here.

The intensity of the flow is inversely proportional to the length of the conductors.—This law has been verified for cotton threads of different lengths by both of the above methods. It only obtains with exactitude where the electric loss in the thread is inconsiderable in reference to the flow itself. Where this condition does not prevail, the author has found that the loss is proportional to the square of the length of the thread, as it ought to be from the law of tensions. According to M. Gaugain, the following are the conditions to be fulfilled in order to obtain results agreeing with Ohm's laws,—laws, that is to say, which ought to apply to the case of a constant flow, and a loss by the air which may be neglected:—

1. A reservoir of great capacity must be added to the electroscope. The author used a condenser.

2. Feeble tensions must not be used, and the initial tension of the electroscope must not differ by more than a few degrees.

3. The conductor must have such dimensions that the flow is considerable.

4. Before commencing each experiment, the tension of the apparatus must be kept constant for some time, in order that the conducting wire may attain a permanent state.

Variation of the tension in different parts of the conductor.—The tension of the string, which at one end is connected with the electroscope, and at the other with the ground, has half the tension of the source. It has been found, moreover, that, by employing the method of discharges, the *dynamic charge* of a string, that is to say, the quantity of electricity with which it is charged while the discharge takes place, is half the *static charge*, or the charge which it has while in connexion with the source of electricity not insulated from the ground.

The laws of *derived* currents have been verified by the same method, by connecting the electroscope with the ground by several strings simultaneously, and by forked strings.

The intensity of the flow is proportional to the tension of the source, if the extremity of the string is in contact with the ground.—The gold-leaf electroscope was graduated so as to determine two tensions, which were in the ratio of 1 to 2; and the method of discharges showed that the intensity of the flow was perceptibly in the same ratio.

The intensity of the flow is proportional to the section of the conductor.—A first series of experiments showed that the flow did not vary, when the section of the conductor was constant, even when the surface varied. With cotton threads, whether insulated or bound together, the intensity of the flow was not modified. The same result was obtained with a silk ribbon, which was successively folded so as to form a plain surface, rolled several times on itself, and lastly rolled once so as to form a cylindrical sac.

A second series of experiments was made by using columns of oil of different lengths and sections, and measuring each time the corresponding flows.

On the dynamic charge.—The meaning of this term has been explained, and it has been seen that it is double the statical charge. This result agrees with Ohm's law on the distribution of tensions on the course of a conductor, and shows at the same time that the dynamic charge, like the statical, is distributed on the surface of the conductor. The author has endeavoured to confirm this principle by other experiments, showing that the dynamic discharge depends on the surface of the conductor, and not, as with the flow, on the section solely. Thus the dynamic discharge of ten insulated cotton threads bears to these threads united in a bundle the ratio 2 to 1. Analogous results have been obtained by using silk ribbons in different shapes.—*Annales de Chimie et de Physique*, vol. lix. p. 5; *Bibliothèque Universelle* for June 1860, p. 146.

WOOD'S FUSIBLE METAL.

Dr. B. Wood, of Nashville, U.S., has secured a patent for an alloy composed of cadmium, tin, lead and bismuth, which fuses at a temperature between 150° and 160° F. The constituents of this *fusible*

metal may be varied according to the other desired qualities of the alloy, viz. cadmium, one to two parts; bismuth, seven to eight parts; tin, two parts; lead, four parts. It is recommended as being especially adapted for all light castings requiring a more fusible material than Rose's or Newton's "fusible metal," it having the advantage of fusing at more than 40° F. lower temperature than these alloys, and, owing to this property, may replace many castings heretofore made only with amalgams. Its fusing-point may be lowered to any extent by the addition of mercury, which may be employed, within certain limits, without materially impairing the tenacity of the metal. In a letter to the Editors, dated Nashville, June 9th, 1860, Dr. Wood says, —

"One point in particular that strikes me as being worthy of note, is the remarkable degree in which cadmium possesses the property of promoting fusibility in these combinations. The alloy of one to two parts *cadmium*, two parts lead, and four parts tin is considerably more fusible than an alloy of one or two parts *bismuth*, two parts lead and four parts tin; and when the lead and tin are in larger proportion the effect is still more marked. It takes *less* cadmium to reduce the melting-point a certain number of degrees than it requires of bismuth, besides that the former does not impair the tenacity and malleability of the alloy, but increases its hardness and general strength.

"Bismuth has always held a preeminent rank among metals as a fluidifying agent in alloys. Its remarkable property of 'promoting fusibility' is specially noted in all our works on chemistry. But I do not find it intimated in any that cadmium ever manifests a similar property. The fact indeed appears to have been wholly overlooked—owing perhaps to the circumstance that as an alloy with certain metals cadmium does not promote fusibility.

"Cadmium promotes the fusibility of some metals, as copper, tin, lead, bismuth, while it does not promote the fusibility of others, as silver, antimony, mercury, &c. (*i. e.* does not lower the melting-point beyond the mean). Its alloy with lead and tin in any proportion, and with silver and mercury within a certain limit, say, equal parts, and especially if two parts silver and one of cadmium or two parts cadmium and one of mercury are used, are tenacious and malleable, while its alloys with *some malleable* metals (gold, copper, platinum, &c.), and probably with *all brittle* metals, are 'brittle.'

"I notice a great discrepancy among authors as to the melting-point of this metal. It is usually put down the same as that of tin (442° F.). Brande (Dict. of Science and Arts) says it 'fuses and volatilizes at a temperature a little below that at which tin melts.' Daniell (according to the New American Cyclopædia) gives its melting-point at 360° F.; while Overman places it at 550°, and gives 600° as the temperature at which it volatilizes.

"The latter is doubtless the nearest the truth. The metal requires for its fusion a temperature too high for measurement by the mercurial thermometer; but from relative tests with other metals I should place its melting-point in round numbers at 600° F., as it melts and

congeals nearly synchronously with lead, the melting-point of which is stated by different authorities as 594° , 600° , and 612° F. It volatilizes at a somewhat higher heat.

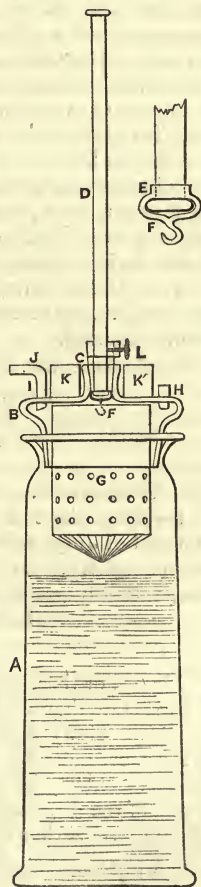
"I draw attention to these facts, believing that the metal possesses properties valuable to art and interesting to science, and that it merits more thorough investigation than appears to have been bestowed upon it."—Silliman's *American Journal* for September 1860.

DESCRIPTION OF AN APPARATUS FOR GENERATING HYDROGEN,
CARBONIC ACID, AND SULPHURETTED HYDROGEN. BY G.
GORE, ESQ.

Having frequently during the last two years found the following apparatus of great convenience in generating hydrogen, carbonic acid, and sulphuretted hydrogen for lectures and other purposes, I beg leave to submit a description of it to the readers of the *Philosophical Magazine*.

A is a large glass jar with a capacious hollow glass stopper B (shown in section) fitted perfectly gas-tight; C is a perforated and greased cork fitted into the central hole of the stopper; a cylindrical rod of glass, D, of very uniform diameter, and slightly enlarged at the ends to a knob-like form, passes tightly through the cork, and supports by means of a ring E and hook F (see separate sketch), a copper or leaden bucket G; the bucket is perforated at its sides, and has also radial slits in its conical lower end to admit the acid water; the hollow stopper has two small openings—H to receive a cork, and I to receive a bent exit-tube, J, for the escape of the gas; K and K' is a section of an annular leaden weight placed for the purpose of preventing the pressure of the enclosed gas lifting the stopper; and L is a small clamp-screw to prevent the rod D slipping downwards by accident. The rod D should be rather thick, to prevent risk of fracture; and the brass ring E, which supports the hook F, should be sufficiently loose upon the rod to turn round freely. The different parts of the sketch are drawn of their relative sizes.

For hydrogen a copper bucket is used, and for sulphuretted hydrogen a thin leaden one. The bucket and its contents, *i. e.* granulated zinc, chalk, or sulphide of iron, in small fragments, can be readily lowered and sustained at any depth of immersion in the acid water, and a steady flow of gas obtained: for hydrogen I have most frequently used a mixture of 2 measures of hydrochloric acid and $1\frac{1}{2}$ of water, or a



previously cooled mixture of 1 measure of oil of vitriol and 5 of water; and for purifying it, 1st, a solution of potash; 2nd, a solution of protonitrate of mercury; and 3rd, sticks of caustic potash. The second purifier was a three-necked Woulfe's bottle, to the central neck of which was fitted, 1st, a Welter's tube containing 3 inches of mercury, to indicate the pressure of gas; and 2nd, a bent tube with a thin vulcanized india-rubber bag at its extremity: the bag received any excess of gas which could not readily escape at the burner, and its elasticity effectually prevented the pulsations and irregularities in the jet of burning gas caused by the bubbling through the liquids.

The most effectual gas-burner I have used has been formed of a piece of iron gas-tubing, 8 inches long and half an inch in diameter, closed at one end by a very thin plate of iron, in which were drilled seven *exceedingly fine* holes. The holes require to be occasionally cleaned by means of a very small steel broach*.

With a generator containing $1\frac{1}{2}$ gallon of acid water and 3 pounds of zinc, and with a pressure of gas of 1 pound per square inch, I have obtained a splendid pencil of flame 20 inches long and $\frac{5}{8}$ ths of an inch in diameter, which quickly melted copper wire $\frac{1}{10}$ th of an inch thick, and melted moderate-sized platinum wire. I have found it of especial value in the analysis of infusible silicates, as a white heat is obtained at a moment's notice, and conveniently maintained for half an hour or more. A ball of spongy platinum held about 4 or 5 inches above the burner when the gas is issuing at a pressure of 1 pound per square inch, produces a loud roaring noise and a singular glow of light without igniting the gas down to the burner, and forms an interesting experiment.

The chief advantage which this apparatus appears to me to possess over others, is the convenience with which it may be used when suddenly required.

Birmingham, Oct. 22, 1860. _____

ON NEW FORMS OF ACTINOMETERS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

21 Ainslie Place, Edinburgh,
August 5, 1860.

I send a short Appendix to my paper on Actinometers in your Journal of July, which, as more distinctly explaining some points as to registry, and more particularly as to *actinometry by phosgene gas*, you may perhaps consider worthy of insertion.

I remain, Gentlemen, your obedient Servant,

C. J. BURNETT.

First. For the benefit of those who have not seen my paper of 1858, I must state that although I have described a mode of applying photographic registry to a continuously acting gas-evolving actinometer, it must be understood that there are great difficulties connected with it,—particularly in determining what would be the direction on the paper of the diagonally rising line which would be the representative of a uniform actinic action, influenced as it must be by the continually

* A plate of platinum instead of iron would probably obviate this necessity, and be an improvement.

decreasing strength of the sensitive liquid, and also by the phenomena which may be roughly characterized as persistence in chemical rest, and persistence in a state of chemical change in the sensitive liquid. Till the errors arising from these sources can be determined and allowed for, such *photographic registry* can be of little use. By operating, however, not continuously with one portion of liquid, but with *separate* portions *successively* uncovered for a fixed period, and again covered up by a proper clockwork or similar arrangement, all difficulty as to registry is removed. Supposing, for instance, that such a compound actinometer has been at work for a day of twelve hours, registering successive hours or half hours, the observer has only to note at the expiry of the twelve hours, or before refilling and setting the instrument to work again, at what point the oil in the register-tube of *each* of the twelve (or of the twenty-four) *separate* actinometers stands. Photographic assistance is hardly required. Still, for very compound instruments, intended to work for a long period without attendance, it may be usefully employed; and even in other cases it is quite possible that the phenomena connected with persistent rest and action of the sensitive liquid may render its employment advantageous; and this can be readily managed, *e. g.*, by making the same clockwork which shuts up each separate actinometer, immediately before it shuts it, uncover a slip of photographic paper behind it. Another plan would be to have the full number, corresponding to the periods to be registered, of sensitive-liquid reservoirs; but instead of giving *each* its own register-tube, to have them all communicating with one registry-liquid reservoir and register-tube. This would simplify much the photographic registry, which would be effected by making the register-tube, as elsewhere, close a slit, and having the paper moved past behind it by a jerk at every half hour or other period, corresponding with that at which the sensitive reservoirs are closed. The exact amount of rise due to each reservoir's exposure would be thus shown on the paper.

Second. Actinometry by means of a mixture of carbonic oxide and chlorine having been in my paper only alluded to in passing, I may now state that this mixture has some advantages over that of *hydrogen* and chlorine. In the first place we have no risk of explosion, the gases uniting quietly even in sunshine. In the second place we have a ready means of estimating the amount of action which has taken place by the condensation effected, as the two gases, in combining to form phosgene or chlorocarbonic acid gas, shrink to half their previous volume. To render this condensation conveniently visible, we have only to carry a tube from the reservoir containing the mixed gases into a vessel containing *eupion*, or some similar liquid, which will neither absorb nor in any way be acted on by them,—the absorption being of course indicated by its rise, and the exact amount of that being easily shown by the attachment of a scale. It is, I think, unnecessary to describe the various modifications of this instrument which suggest themselves, or to say more than that the same plans of combining a number of them into one compound instrument for registry of successive intervals, which we have indicated as applied to the sensitive-liquid actinometer, are

equally applicable here, as are also the same contrivances for photographic registry, with trifling alterations corresponding to the difference between expansion and condensation of gas.

Having alluded to the application of convex lenses to increase the sensitiveness of actinometers, I may as well add that, to avoid the concentration of heat as well as light, we must, at least when operating with the sun's rays, make them pass, either before entering or after leaving the lens, through some medium which will intercept (or metamorphose) the heat rays, *e. g.* blue glass or coloured solutions, or some one of those salts (either solid or in solution) which are known for their adiathermic properties, or else we may have the lens itself constructed of one of these substances.

In conclusion, the amount of success which has already attended the efforts to measure what we may for convenience call the cyanic actinism, suggests the desirableness of attempts being made to obtain some similar system of registry (either by properly prepared papers, or liquids, or gases) of the strength of that opposite influence lower down in the spectrum which has been made known to us by Dr. Draper and Mr. R. Hunt; and our success thus far may then suggest further the possibility of our being able separately to identify and to contrive some measurement for some of the many other powers probably contained in the light of the sun and other luminaries, both as originally emitted, and as altered by reflexion or transmission.

ARSENIC IN COAL.

At a recent meeting (Oct. 16) of the Manchester Literary and Philosophical Society, Dr. R. Angus Smith gave a short account of his examination of coal pyrites for arsenic. He stated that although the knowledge of the existence of arsenic in the iron pyrites found in coal may not be considered perfectly novel, it certainly does not seem to be known that arsenic is so widely disseminated as to form an ordinary constituent of the coals burnt in our towns; and chemists of celebrity have held, and now hold, it to be absent there. He had examined fifteen specimens of coal in Lancashire, and found arsenic in thirteen. He had also found it in a few others; but Mr. Binney having promised a collection, properly arranged, the examination will then be made more complete. Mr. Dugald Campbell had also lately found arsenic in coal pyrites; this had a very direct bearing on our sanitary knowledge, as we must now be obliged to add arsenic to the number of impurities in the atmosphere of our large towns. It is true that he had not actually obtained it from the atmosphere; but when the pyrites is burnt the arsenic burns and is carried off along with the sulphur. One or two coal brasses (as they are called) contained copper, a metal that is also to some extent volatilized, as may be readily observed wherever copper-soldering takes place. Although an extremely small amount of copper is carried up from furnaces, it is not well entirely to ignore it. The amount of arsenic, however, is probably not without considerable influence; and we may probably learn the reason why some towns seem less affected than others by the burning of coals, by examining the amount of arsenic burnt as well as sulphur.

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

DECEMBER 1860.

LV. *On the Form of Satellites revolving at small distances from their Primaries.* By DANIEL VAUGHAN*.

SECONDARY planets which revolve at comparatively small distances from great central orbs, experience very singular effects from a kind of disturbing action, which is either wholly imperceptible on the primaries themselves, or is only manifested in producing an alternate rise and fall of their extensive oceans. The powerful attraction of Jupiter must cause his nearest satellite to feel a tidal force several thousand times greater than that which every day swells our terrestrial waters; and the variation of gravity on the surface of the small world, from this disturbance alone, may be estimated as nearly five times as great as our own globe exhibits in consequence of its diurnal movement. In the Saturnian system, where many attendants are much closer to the primary, they must be more seriously affected by its unequal attraction on their parts. Were the size of the orbit below a certain limit, the disturbing influence of the central body would so far neutralize the attractive power of the satellite in two directions as to render it incapable of maintaining the usual planetary form; and it appears that, in the zone occupied by Saturn's rings, no large secondary planets could roll in security unless they were considerably more dense than the kind of matter which predominates in that part of the solar domain. During the year 1853, in announcing the result of my researches on this subject, I ascribed the existence of the rings to their proximity to Saturn; and, though the theory has been slow in receiving attention, I hope that it may now claim a degree of interest sufficient to justify me in submitting to scientific men, in the

* Communicated by the Author.

present article, some mathematical investigations of the chief cases of planetary instability.

The peculiar arrangement which is supposed to prevail in all secondary systems, for keeping the same sides of satellites always turned to their primary, is well calculated to preserve these second-rate worlds from the injurious effects of excessive tides, since they must always have their oceans elevated and their forms elongated at the same localities. The exact synchronism of the rotary and the orbital motion mainly contributes to secure this important end; but, to remove entirely the dynamic effects of the great disturbance, it is necessary that the satellite should rotate around an axis perpendicular to the plane in which it revolves, and that its path should be a true circle. In describing an ellipse sufficiently small for the production of a very great disturbing force, the planetary structure would have its safety much endangered by the oscillatory movements of its parts; and the oscillations would be attended with more fatal results if, from a want of the other conditions, it presented different sides alternately to the primary. In calculating the dimensions of the smallest orbit in which it is possible for a secondary planet to hold its parts together by the tie of gravity, we are necessarily restricted to the cases most favourable for stability; and I shall accordingly suppose that the body has its axis perpendicular to the plane of its orbit, that the latter is exactly circular, and that the rotation takes place in the same time and in the same direction as the periodical revolution. In such circumstances the relative direction of the primary from every part of the satellite must ever remain unchanged, and its powerful attraction must be productive of the least derangement on the surface of the latter body.

In a very small orbit there appears to be a physical necessity for a synchronism of the orbital and rotary movements of a satellite similar in constitution to our globe; for the rotation would be gradually changed by the action of enormous tides, until it finally occupied the same time as the orbital revolution. Nor can our conclusion be very different if we agree with Lagrange in ascribing the arrangement to the deviation of these humble worlds from true spheres, and to the consequent tendency of their longest diameters to point towards the central body. In two secondary planets of the same size, form, and density, this tendency would be inversely proportional to the fourth power of their distances from the primary; but were the ellipticity of both bodies such as the attraction of the latter might occasion on a yielding solid mass, the tendency would be in inverse proportion partly to the seventh, and partly to the higher powers of the same quantities. There are accordingly sufficient grounds for

the hypothesis I have adopted; and it would seem that in this, as in many other problems of physical science, the cases which occur in nature are such as the powers of analysis can reach with the greatest facility.

To begin with the case of a solid spherical satellite, which I shall suppose to be capable of preserving its form unchanged by the disturbance previous to the final rupture: let r denote its radius, r' the radius of the primary, and g the measure of the attraction exerted at the distance k by a portion of matter taken as unity, and of equal density with the satellite, but immeasurably inferior to it in magnitude. Put π for 3.1416; R for the radius of the spherical space which the matter of the primary might fill if reduced to the same density as the satellite; and x for the distance between the centres of both bodies. In the absence of every disturbing influence, the force of gravity on the surface of the satellite will be

$$\frac{4gk^2\pi r}{3} \dots \dots \dots (1)$$

The diminution of this gravity at the point nearest to the primary, by the attraction of the latter, is equal to

$$\frac{4gk^2\pi R^3}{3(x-r)^2} - \frac{4gk^2\pi R^3}{3x^2},$$

or

$$\frac{4gk^2\pi R^3}{3x^3} \left(2r + \frac{3r^2}{x} + \frac{4r^3}{x^2} + \frac{5r^4}{x^3} \&c. \right) \dots \dots (2)$$

At 90 degrees from this point, gravity is augmented by the quantity

$$\frac{4gk^2\pi R^3}{3x^3} \left(r - \frac{3r^3}{2x^2} + \frac{15r^5}{8x^4} \&c. \right) \dots \dots \dots (3)$$

Now if M and m denote the attractive force of both bodies at the distance k , the period of the satellite's revolution (which I shall call T) will be expressed by the formula

$$T = \frac{2\pi x^{\frac{3}{2}}}{k \sqrt{M+m}} \dots \dots \dots (4)$$

As its time of rotation is also equal to T , the equatorial velocity of the satellite is $= \frac{kr \sqrt{M+m}}{x^{\frac{3}{2}}}$. Calling this v , the centrifugal

force at the equator will be equal to $\frac{v^2}{r}$, or

$$\frac{k^2(M+m)r}{x^3} \dots \dots \dots (5)$$

Substituting for M and m their values, $\frac{4g\pi R^3}{3}$ and $\frac{4g\pi r^3}{3}$, this becomes

$$\frac{4gk^2\pi(R^3+r^3)r}{3x^3} \dots \dots \dots (6)$$

Subtracting the sum of the expressions (2) and (6) from (1), there results the equation

$$\frac{4gk^2\pi r}{3} - \frac{4gk^2\pi R^3}{3x^3} \left(3r + \frac{3r^2}{x} + \frac{4r^3}{x^2} + \frac{5r^4}{x^3} + \frac{r^4}{R^3} \&c. \right) = F, \quad (7)$$

in which F represents the force of gravity at that part of the satellite in conjunction with the primary. If, instead of adhering to the hypothesis I have adopted, we suppose the axis of rotation to form an oblique angle with the plane of the orbit, F will be variable, and formula (7) will express only the lowest limit which it attains during every revolution. Were the satellite homogeneous and very inferior to the primary in magnitude, all the matter between its centre and any point on its surface must lose weight in nearly the same ratio by the disturbing force.

By making F in equation (7) equal to nothing, we obtain

$$x = R \sqrt[3]{3} + \frac{r}{3} + \frac{2r^2}{9R \sqrt[3]{3}} + \frac{32r^3}{81R^2 \sqrt[3]{9}} \&c. \dots \dots (8)$$

This value of x represents the radius of the circular orbit in which a spherical satellite would be incapable of retaining disconnected bodies at the place next the primary, and could give only a very insignificant weight to the matter on a direct line between this and the opposite part of its surface. In the case of the earth and moon, the value of x in equation (8) would be a little over 7500 miles; and were the lunar orb made to revolve in a circle of so small a radius, gravity would disappear at the part of its surface nearest to us, so that there could be no adequate counterpoise to the enormous pressure arising from the weight of matter in other localities. It therefore becomes necessary to correct our hypothesis respecting the figure of a satellite in such circumstances. A far less degree of proximity to our globe would be sufficient to give the moon a very considerable distortion from a true sphere; and as their cohesive force, when once overcome, could oppose little resistance to their arrangement, the lunar materials would ultimately exhibit a form differing little from that which a fluid might assume under the operation of the same forces.

In taking up the case in which the satellite is to be regarded as fluid, I think it advisable to have recourse to an approximate method of investigation, which may be conveniently extended for the attainment of any desirable degree of accuracy, while it

serves to remove a peculiar difficulty attending the solution of problems of this character. In his investigations respecting the stability of a fluid planet as dense as the earth, Laplace finds that a rotation performed in less than $\cdot 10090$ of a day would be incompatible with its existence in a spheroidal figure; but as its equatorial gravity is only partially neutralized by centrifugal force, he concludes that such a rapidly rotating mass would remain undivided under some form which analysis does not reveal. A complete cessation of gravity in certain directions is not, however, essential to an unstable equilibrium, either in the case considered by Laplace, nor in the one now under consideration, which seems to be better suited for the removal of the difficulties of questions of this nature, as the time of rotation is prevented from changing with the alterations in the figure of the body. The stability of a fluid satellite, consigned to a small orbit and elongated in the direction of its primary, is evidently at an end—not when gravity is wholly suppressed at the parts through which the greatest diameter passes, but when the force with which a column of matter coincident with this line presses to the centre ceases to acquire, from a further elongation of the body, any preponderance over the pressure of a similar column of matter extending from the centre to the nearest point of the surface. Thus putting P and P' for the pressures which the central region sustains from two similar columns of matter extending along the greatest and the least diameters, and ϵ for the excentricity of the section made by a plane passing through both and supposed to be an ellipse, the distance from the primary when dismemberment must occur, and the greatest deviation from a sphere, may be determined from the equations

$$P - P' = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$\frac{dP}{d\epsilon} - \frac{dP'}{d\epsilon} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Another method of arriving at the same result will appear when we consider that a satellite, in this critical situation, must have its ellipticity increased to an enormous extent by a slight increase of the disturbance or by a small diminution of the distance; and instead of (10) we may have recourse to the equations

$$\frac{d\epsilon}{dx} = \text{infinity, or } \frac{dx}{d\epsilon} = 0, \quad . \quad . \quad . \quad (11)$$

where x , as in the previous notation, represents the radius of the orbit.

Were the satellite small compared with the central sphere, it could not deviate much in form from an ellipsoid, the minor and mean axes of which differ by a quantity much less than the

excess of the major axis over either of them. They may therefore be considered equal, at least in the first approximation; and if greater accuracy be required, it may be obtained by introducing corrections depending on the square, the fourth, and the sixth powers of the excentricity of the elliptical section in which both are situated. Accordingly, let the satellite be regarded as a homogeneous prolate spheroid; and let A and B represent the major and minor semiaxes, the former of which always ranges with the primary's centre. To find the attraction at the extremity of the major axis, conceive the body to be divided into innumerable sections by planes passing through this line, while these sections are subdivided into a corresponding number of infinitely small pyramids, having their vertices all terminating at the proposed point, which shall be taken as the origin of the coordinates. Put θ for the angle which any section makes with the plane of the orbit, l for the length of any of its pyramids whose inclination to the major axis is denoted by ϕ . The vertical angles of this minute pyramid will be $d\phi$ and $d\theta \sin \phi$, and the component of its attraction in the direction of the major axis is

$$gk^2 l \sin \phi \cos \phi d\phi d\theta. \quad . \quad . \quad . \quad (12)$$

Substituting for l its value $\frac{2AB^2 \cos \phi}{A^2 \sin^2 \phi + B^2 \cos^2 \phi}$, this expression becomes

$$\frac{2gk^2 AB^2 \sin \phi \cos^2 \phi d\phi d\theta}{A^2 \sin^2 \phi + B^2 \cos^2 \phi} \text{ or } \frac{2gk^2 B^2 \sin \phi \cos^2 \phi d\phi d\theta}{A(1 - \epsilon^2 \cos^2 \phi)}; \quad (13)$$

and the double integral of the last quantity, or

$$\frac{2gk^2 B^2}{A} \int d\theta \int \frac{\cos^2 \phi \sin \phi d\phi}{1 - \epsilon^2 \cos^2 \phi}, \quad . \quad . \quad . \quad (14)$$

taken within the proper limits, will represent the attraction of the satellite at the extremity of the major axis. Now

$$\int \frac{\cos^2 \phi \sin \phi d\phi}{1 - \epsilon^2 \cos^2 \phi} = \frac{\cos \phi}{\epsilon^2} - \frac{1}{2\epsilon^3} \log \frac{1 + \epsilon \cos \phi}{1 - \epsilon^2 \cos \phi} + C. \quad (15)$$

When this is taken within the limits of $\phi = \theta$ and $\phi = \frac{\pi}{2}$, and substituted in (14), the latter becomes

$$\frac{2gk^2 B^2}{A} \int d\theta \left(\frac{1}{2\epsilon^3} \log \frac{1 + \epsilon}{1 - \epsilon} - \frac{1}{\epsilon^2} \right); \quad . \quad . \quad (16)$$

and making a second integration between the limits of $\theta = +\pi$ and $\theta = -\pi$, the expression for the attractive force is

$$\frac{4gk^2 \pi B^2}{A} \left(\frac{1}{2\epsilon^3} \log \frac{1 + \epsilon}{1 - \epsilon} - \frac{1}{\epsilon^2} \right). \quad . \quad . \quad (17)$$

In estimating the effects of centrifugal force and the disturbing action of the primary in diminishing this attraction, we may use, with some modifications, the formulas already obtained for the case of a spherical satellite. The entire diminution arising from both disturbances may be found by adding formulas (2) and (5), substituting A for r in the result, and retaining only the term containing the first powers of A , the rest of the series being small in comparison to it. There results the amount

$$\frac{4gk^2\pi AR^3}{x^3} \text{ nearly; } \dots \dots \dots (18)$$

and this being subtracted from (17), we obtain

$$\frac{4gk^2\pi B^2}{A} \left(\frac{1}{2\epsilon^3} \log \frac{1+\epsilon}{1-\epsilon} - \frac{1}{\epsilon^2} \right) - \frac{4gk^2\pi AR^3}{x^3} = G, \quad (19)$$

the approximate value of the force of gravity at the points nearest to the primary and most distant from it. To find the weight or the pressure of a uniform column of the fluid extending from either locality to the central region of the satellite, denote by S the distance of any part from the centre; the force of gravity operating on it will be $\frac{GS}{A}$, and dP is equal to $\frac{GSdS}{A}$; whence

$$P = \frac{S^2G}{2A}. \quad \dots \dots \dots (20)$$

Taking this integral within the limits of $S=0$ and $S=A$, and substituting for G its value from equation (19), there results

$$P = 2gk^2\pi B^2 \left(\frac{1}{2\epsilon^3} \log \frac{1+\epsilon}{1-\epsilon} - \frac{1}{\epsilon^2} \right) - \frac{2gk^2\pi A^2R^3}{x^3}, \quad \dots \quad (21)$$

in which P represents the pressure when the transverse section is unity.

To determine the pressure of a similar column of matter coincident with the axis of rotation, let either extremity of this line be taken as the origin of three coordinate planes,—one being tangent to the spheroid at that point, the second vertical and ranging with the primary, and the third perpendicular to the line in which both planes intersect. Through this line of intersection let planes be conceived to pass, dividing the body into an infinite number of sections which are subdivided into infinitely small pyramids, whose vertices all terminate at the origin of the coordinates. If θ denote the inclination of any section to the second plane, and ϕ the inclination of any of its pyramids to the third, the length of the pyramid being l , the angles at its vertex will be $d\phi$ and $d\theta \cos \phi$, and the vertical component of its attractive force will be

$$gk^2l \cos^2 \phi \cos \theta d\phi d\theta. \quad \dots \dots \dots (22)$$

This expression, on substituting for l its value

$$\left(\frac{2A^2B \cos \phi \cos \theta}{A^2 \cos^2 \phi + B^2 \sin^2 \phi} \right),$$

becomes

$$\frac{2gk^2A^2B \cos^3 \phi \cos^2 \theta d\phi d\theta}{A^2 \cos^2 \phi + B^2 \sin^2 \phi},$$

or

$$\frac{2gk^2B \cos^3 \phi \cos^2 \theta d\phi d\theta}{1 - \epsilon^2 \sin^2 \phi}; \quad . \quad . \quad . \quad . \quad . \quad (23)$$

and the attraction of the spheroid at the point in question will be

$$2gk^2B \int \cos^2 \theta d\theta \int \frac{(1 - \sin^2 \phi) \cos \phi d\phi}{1 - \epsilon^2 \sin^2 \phi}. \quad (24)$$

The last integral, or

$$\int \frac{(1 - \sin^2 \phi) \cos \phi d\phi}{1 - \epsilon^2 \sin^2 \phi} = \frac{\sin \phi}{\epsilon^2} - \frac{1 - \epsilon^2}{2\epsilon^3} \log \frac{1 + \epsilon \sin \phi}{1 - \epsilon \sin \phi},$$

which becomes, on taking ϕ within the limits of $+\frac{\pi}{2}$ and $-\frac{\pi}{2}$,

$$\frac{2}{\epsilon^2} - \frac{1 - \epsilon^2}{\epsilon^3} \log \frac{1 + \epsilon}{1 - \epsilon}. \quad . \quad . \quad . \quad . \quad . \quad (25)$$

Substituting this value for the last integral in (24), and making a second integration, there results

$$4gk^2B \left(\frac{\sin 2\theta}{4} + \frac{\theta}{2} \right) \left(\frac{1}{\epsilon^2} - \frac{1 - \epsilon^2}{2\epsilon^3} \log \frac{1 + \epsilon}{1 - \epsilon} \right);$$

and this, taken within the limits of $\theta = +\frac{\pi}{2}$ and $-\frac{\pi}{2}$, becomes

$$2k^2g\pi B \left(\frac{1}{\epsilon^2} - \frac{1 - \epsilon^2}{2\epsilon^3} \log \frac{1 + \epsilon}{1 - \epsilon} \right). \quad . \quad . \quad . \quad (26)$$

This expresses approximately the attractive energy of the body at the extremity of its axis of rotation. The augmentation of gravity which the disturbance occasions at this locality may be deduced from formula (3), which, on substituting B for r , and retaining only the first term of the series, becomes

$$\frac{4gk^2\pi BR^3}{3x^3}. \quad . \quad . \quad . \quad . \quad . \quad (27)$$

Adding this to (26), we arrive at the following equation for the value of G' , which shall be used to represent the force of gravity at the poles of the satellite,

$$G' = \frac{4gk^2\pi BR^3}{3x^3} + 2gk^2\pi B \left(\frac{1}{\epsilon^2} - \frac{1 - \epsilon^2}{2\epsilon^3} \log \frac{1 + \epsilon}{1 - \epsilon} \right). \quad . \quad (28)$$

A course similar to that pursued in deducing formulas (20) and

(21) will enable us to derive from (28) an equation for the pressure of a fluid column having its section equal to unity, and extending from the centre to either of the poles. Denoting by P' its pressure at the centre,

$$P' = \frac{2gk^2\pi B^2 R^3}{3x^3} + gk^2\pi B^2 \left(\frac{1}{\epsilon^2} - \frac{1-\epsilon^2}{2\epsilon^3} \log \frac{1+\epsilon}{1-\epsilon} \right). \quad (29)$$

Now, in accordance with the condition of equilibrium expressed in formula (9), make the values of P and P' in equations (21) and (29) equal; substituting for B^2 its equal, $A^2(1-\epsilon^2)$, and dividing by $gk^2\pi A$, we obtain

$$(1-\epsilon^2) \left(\frac{1}{\epsilon^3} \log \frac{1+\epsilon}{1-\epsilon} - \frac{2}{\epsilon^2} \right) - \frac{2R^3}{x^3} = \frac{2(1-\epsilon^2)R^3}{3x^3} + (1-\epsilon^2) \left(\frac{1}{\epsilon^2} - \frac{1-\epsilon^2}{2\epsilon^3} \log \frac{1+\epsilon}{1-\epsilon} \right),$$

which after some reductions and transposition gives

$$\frac{R^3}{x^3} = \frac{3(1-\epsilon^2)}{4-\epsilon^2} \left(\frac{3-\epsilon^2}{4\epsilon^3} \log \frac{1+\epsilon}{1-\epsilon} - \frac{3}{2\epsilon^2} \right). \quad (30)$$

It appears from equation (11), and from the principles on which it has been deduced, that when the stability of the satellite ceases to be possible, $\frac{dx}{d\epsilon}$ is equal to nothing; and accordingly equation (30) and its differential might enable us to determine the size of the smallest orbit such a body could describe, and its deviation from a sphere previous to the final dismemberment. To avoid, however, the difficulties of the resulting equation and the ambiguity of its roots, it will be advisable to make the estimate from a comparison of the following values of $\frac{R^3}{x^3}$, corresponding to different degrees of excentricity, and calculated from formula (30).

Values of ϵ .	Values of $\frac{R^3}{x^3}$.	Values of ϵ .	Values of $\frac{R^3}{x^3}$.
·80	·060500	·86	·064688
·81	·061418	·87	·064924
·82	·062289	·88	·064951
·83	·063080	·89	·064740
·84	·063793	·90	·064239
·85	·064293	·91	·063391

An inspection of this Table shows that the elongation of a satellite increases to a most enormous extent with the disturbance, and that the dismemberment is inevitable when the value

of ϵ is between 87 and 88. From the corresponding value of $\frac{R^3}{x^3}$, it appears that x at this critical period is equal to 2.489 R.

This expresses approximately the least distance from the primary at which a satellite could be preserved. To give greater generality to the formula, designate by D and d the densities of both bodies, r' being, in accordance with the previous notation, equal to the actual radius of primary, and R the radius of the sphere which it might fill if it became as dense as the satellite. It will

be easily seen that $R = r' \sqrt[3]{\frac{D}{d}}$, and accordingly x' is equal to

$$2.489 r' \sqrt[3]{\frac{D}{d}} \dots \dots \dots (31).$$

It appears also to be independent of the actual size of the satellite. If indeed we retained in our investigation the square and the higher powers of r , the result would exhibit a slight difference in favour of the stability of smaller attendants; but this is more than counterbalanced by the superior density of larger bodies composed of the same materials, but capable of compressing them by a greater attractive force.

It appears, therefore, that the laws of equilibrium prevent the existence of satellites over a large space around each primary planet; and it might also be shown that, beyond this region, the existence of planetary rings is equally impossible. The extent of this region will be different for satellites unequally dense, and it may be easily calculated in each case by our last formula. Although our data is somewhat defective in the case which presents itself in the Saturnian system, yet when we calculate the density which a satellite should possess to maintain its planetary form in different parts of the zone occupied by his rings, it seems impossible to resist the conclusion that the condition of this annular appendage is the necessary consequence of its proximity to Saturn.

Cincinnati, October 29, 1859.

LVI. *On the Cubic Centres of a Line with respect to Three Lines and a Line.* By A. CAYLEY, Esq.*

CONSIDER a line L in relation to the three lines X, Y, Z and the line I : through the point of intersection of the lines X, L , draw any line meeting the lines I, Y, Z , and let the harmonic of the intersection with I , in relation to the intersec-

* Communicated by the Author.

tions with Y, Z , be ξ ; then the locus of the point ξ is a conic passing through the points YI, ZI, YZ .

If, in like manner, through the point of intersection of the lines Y, L , there is drawn any line meeting the lines I, Z, X , and the harmonic of the intersection with I , in relation to the intersections with Z, X , is called η , the locus of the point η is a conic passing through the points ZI, XI, ZX .

And so, if through the point of intersection of the lines Z, L there is drawn any line meeting the lines I, X, Y , and the harmonic of the intersection with I , in relation to the intersections with X, Y , is called ζ , then the locus of ζ is a conic passing through the points XI, YI, XY .

The pairs of conics, viz. the second and third, third and first, first and second conics, have obviously in common the points XI, YI, ZI respectively. They besides intersect all three of them in three points, which may be termed the *cubic centres* of the line L in relation to the lines X, Y, Z and the line I .

The line L may be such that two of the three cubic centres coincide; the locus of the coincident centres is in this case a conic which touches the lines X, Y, Z harmonically in regard to the line I ; that is, it touches each of the three lines in the point which is the harmonic of its intersection with I in relation to its intersections with the other two lines.

Except that the line I is there taken to be infinity, the foregoing theorems occur in Plücker's *System der analytischen Geometrie* (Berlin, 1835), p. 177 *et seq.*; and they play an important part in his classification of curves of the third order (see p. 220 *et seq.*). It is, I think, an omission that he has not sought for the curve which is the envelope of the line L in the above-mentioned case of the two coincident centres: I find that the envelope is a curve of the fourth order, having four-pointic contact with the lines X, Y, Z harmonically in regard to the line I ; viz., if the equations of the lines X, Y, Z are $x=0, y=0, z=0$ respectively, and the equation of the line I is $x+y+z=0$, then the equation of the envelope in question is

$$\sqrt[4]{x} + \sqrt[4]{y} + \sqrt[4]{z} = 0,$$

a result which is also interesting as exhibiting a geometrical construction of the curve represented by this equation.

The investigation of the series of theorems is as follows; taking

$x=0$	for the equation of	$X,$
$y=0$,,	$Y,$
$z=0$,,	$Z,$
$x+y+z=0$,,	$I,$
$\lambda x + \mu y + \nu z = 0$,,	$L,$

Then, first, in order to find the curve which is the locus of ξ , the coordinates of the point XL are given by $x : y : z = 0 : \nu : -\mu$; or if, as it is convenient to do, we take X, Y, Z (instead of x, y, z) for current coordinates, by $X : Y : Z = 0 : \nu : -\mu$. Hence taking x, y, z as the coordinates of ξ , the equation of the line through XL, ξ is

$$\begin{vmatrix} X, & Y, & Z \\ x, & y, & z \\ 0, & \nu, & -\mu \end{vmatrix} = 0,$$

viz.

$$X(\mu y + \nu z) - x(\mu Y + \nu Z) = 0.$$

And at the point where this line meets the line I, the equation whereof is

$$X + Y + Z = 0,$$

we have

$$(Y + Z)(\mu y + \nu z) + x(\mu Y + \nu Z) = 0;$$

that is,

$$Y(\mu x + \mu y + \nu z) + Z(\nu x + \mu y + \nu z) = 0.$$

Hence this line, and the line

$$Yz - Zy = 0,$$

with the lines

$$Y = 0, \quad Z = 0,$$

are the lines which pass through the point YZ and the four harmonic points, and they form therefore a harmonic pencil; or we have

$$y(\mu x + \mu y + \nu z) - z(\nu x + \mu y + \nu z) = 0,$$

or what is the same thing,

$$(\mu y - \nu z)(x + y + z) + 2yz(\nu - \mu) = 0,$$

as the locus of the point ξ : the locus is therefore a conic passing through the points YI, ZI, YZ.

The equations of the conics which are the loci of X, Y, Z respectively, are therefore

$$U = (\mu y - \nu z)(x + y + z) + 2yz(\nu - \mu) = 0,$$

$$V = (\nu z - \lambda x)(x + y + z) + 2zx(\lambda - \nu) = 0,$$

$$W = (\lambda x - \mu y)(x + y + z) + 2xy(\mu - \lambda) = 0;$$

and the identical equation,

$$U\lambda x + V\mu y + W\nu z = 0,$$

shows that these conics have three points of intersection in common. The three equations, and a fourth one to which they give rise, may be written

$$\frac{\mu}{z} - \frac{\nu}{y} + \frac{2(\nu - \mu)}{x + y + z} = 0,$$

$$\frac{\nu}{x} - \frac{\lambda}{z} + \frac{2(\lambda - \nu)}{x + y + z} = 0,$$

$$\frac{\lambda}{y} - \frac{\mu}{x} + \frac{2(\mu - \lambda)}{x + y + z} = 0,$$

$$\frac{\nu - \mu}{x} + \frac{\lambda - \nu}{y} + \frac{\mu - \lambda}{z} = 0;$$

and each of these is the equation of a conic passing through the three cubic centres.

If two of the three centres coincide, then the conics all touch at the coincident centres. Consider the first and second conics: these intersect at the point $z=0$, $x+y+z=0$; and the line $x+y+z=2kz$, if k be properly determined, or what is the same thing, the line $x+y+z=\frac{2(\theta+\nu)}{\theta}z$, if θ is properly determined,

will be a line passing through the last-mentioned point and one of the other points of intersection: k or θ will of course be determined by a cubic equation; and if this has a pair of equal roots, the conics will touch. But the equation of the line, combined with those of the two conics, gives

$$x:y:z = \frac{1}{\theta+\lambda} : \frac{1}{\theta+\mu} : \frac{1}{\theta+\nu};$$

and substituting these values in the equation of the line, we have

$$\frac{1}{\theta+\lambda} + \frac{1}{\theta+\mu} + \frac{1}{\theta+\nu} - \frac{2}{\theta} = 0,$$

which is (as it should be) a cubic equation in θ .

If the equation in θ has equal roots, then

$$\frac{1}{(\theta+\lambda)^2} + \frac{1}{(\theta+\mu)^2} + \frac{1}{(\theta+\nu)^2} - \frac{2}{\theta^2} = 0;$$

and putting in these two equations,

$$x = \frac{m}{\theta+\lambda}, \quad y = \frac{m}{\theta+\mu}, \quad z = \frac{m}{\theta+\nu},$$

we have

$$x + y + z - \frac{2m}{\theta} = 0,$$

$$x^2 + y^2 + z^2 - \frac{2m^2}{\theta^2} = 0;$$

or eliminating m ,

$$(x + y + z)^2 = 2(x^2 + y^2 + z^2);$$

that is,

$$x^2 + y^2 + z^2 - 2yz - 2zx - 2xy = 0;$$

or what is the same thing,

$$\sqrt{x} + \sqrt{y} + \sqrt{z} = 0,$$

for the equation of the locus of the coincident centres: such locus is therefore a conic touching the lines $x=0, y=0, z=0$, in the points of intersection with $y-z=0, z-x=0, x-y=0$ respectively; it is a conic touching the lines X, Y, Z harmonically in regard to the line I.

To find the envelope of the line L, the most convenient course is to take the equation in θ in the reduced form

$$\theta^3 - \theta(\mu\nu + \nu\lambda + \lambda\mu) - 2\lambda\mu\nu = 0;$$

this will have a pair of equal roots if

$$(\mu\nu + \nu\lambda + \lambda\mu)^3 - 27\lambda^2\mu^2\nu^2 = 0;$$

that is, if

$$\mu\nu + \nu\lambda + \lambda\mu - 3(\lambda\mu\nu)^{\frac{2}{3}} = 0;$$

or if

$$\frac{1}{\lambda} + \frac{1}{\mu} + \frac{1}{\nu} - 3\frac{1}{(\lambda\mu\nu)^{\frac{1}{3}}} = 0;$$

or finally, if

$$\lambda^{-\frac{1}{3}} + \mu^{-\frac{1}{3}} + \nu^{-\frac{1}{3}} = 0,$$

which is the relation between λ, μ, ν in order that the line

$$\lambda x + \mu y + \nu z = 0$$

may have two coincident centres; this gives at once for the equation of the envelope

$$\sqrt[4]{x} + \sqrt[4]{y} + \sqrt[4]{z} = 0,$$

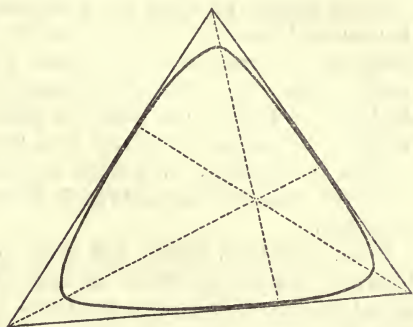
which is the equation of a curve of the fourth order having four-pointic contact with the lines $x=0, y=0, z=0$, at the points of intersection with the lines $y-z=0, z-x=0, x-y=0$ respectively, *i. e.* it has four-pointic contact with the lines X, Y, Z harmonically in regard to the line I.

It may be noticed that the rationalized form of the equation $\sqrt[4]{x} + \sqrt[4]{y} + \sqrt[4]{z} = 0$ is

$$x^4 + y^4 + z^4 - 4(yz^3 + y^3z + zx^3 + z^3x + xy^3 + x^3y) + 6(y^2z^2 + z^2x^2 + x^2y^2) - 124(x^2yz + y^2zx + z^2xy) = 0.$$

If, to fix the ideas, the signs of the coordinates x, y, z are so determined that a point *within* the triangle $x=0, y=0, z=0$ has its coordinates positive (in which case the line $x+y+z=0$ will cut the three sides *produced*), the curve $\sqrt[4]{x} + \sqrt[4]{y} + \sqrt[4]{z} = 0$ will lie wholly within the triangle, and will be of the form shown

by the annexed figure. This is, in fact, the form of the curve in the case considered by Plücker, where the line is at infinity, the points of contact being the middle points of the sides. And his five groups of curves, α , β , γ , δ , ϵ , and two subdivisions of the group β (see pp. 221–224), cor-



respond to the following positions of the line in regard to the triangle and curve, viz.—

- α . The line cuts the three sides produced.
- β . It passes through an angle, (*a*) cutting, or (*b*) not cutting the curve.
- γ . It cuts two sides and a side produced, but does not cut or touch the curve.
- δ . It cuts two sides and a side produced, and touches the curve.
- ϵ . It cuts two sides and a side produced, and cuts the curve.

It is hardly necessary to remark that, in the general case, the tangential equation of the curve is

$$\xi^{-\frac{1}{3}} + \eta^{-\frac{1}{3}} + \zeta^{-\frac{1}{3}} = 0;$$

or what is the same thing,

$$(\eta\zeta + \zeta\xi + \xi\eta)^3 - 27\xi^2\eta^2\zeta^2 = 0;$$

and that the curve is therefore of the sixth class.

2 Stone Buildings, W.C.,
October 16, 1860.

LVII. *On Darwinite, a new Mineral Species from Chile.*

By DAVID FORBES, F.R.S. &c.*

THIS mineral, a specimen of which was given me as being native arsenic, is stated to occur near Potrero Grande, not many miles to the south-east of the town of Copiapo in Northern Chile, where it is said to present itself as small veins or strings cutting through the porphyritic claystones which form the mountain range at that place, and which represent the upper oolitic formation in geological age.

* Communicated by the Author.

These strings or veins are described as entirely composed of this mineral in a state of great purity, but very narrow, seldom attaining the breadth of more than 2 inches across. The specimen which was examined appeared to represent the entire thickness of such a vein, and is a mass of about $1\frac{1}{2}$ inch across, composed of the pure mineral free from gangue or extraneous metallic compounds, but having its sides coated with red oxide of copper, and in some parts spots of a green arsenite or arseniate of copper.

From its colour, lustre, and great specific gravity, it had, on discovery, been supposed to be native silver, and workings were accordingly commenced on the veins,—which, however, were at once abandoned when the assays made at Copiapo showed the very small amount of silver entering into its composition; and as when thrown into a forge it evolved arsenical fumes in abundance, it was regarded as “Arseniko,” or native arsenic, and, strange enough, does not even appear to have been assayed for copper.

The mineral is massive, without trace of cleavage, is rather brittle; but although easily broken, its surface may be distinctly impressed by the hammer before yielding: fracture even; hardness 3.5.

Lustre metallic; colour of freshly-fractured surface dark silver-grey, tarnishing on exposure to a dirty bronze-yellow; streak metallic, dark silver-grey; opaque.

The specific gravity of three separate fragments was found to be respectively 8.69, 8.67, and 8.57; consequent mean 8.64.

Heated in a closed tube, the mineral does not alter, or at most a faint trace of arsenious acid sublimes on to the side of the tube, arising from the action of the air contained in the same; in an open tube, a distinct white sublimate of arsenious acid is obtained.

Before the blowpipe on charcoal in reducing flame, it fuses readily to a silver-white globule, which in the act of cooling evolves arsenical fumes and becomes slightly red on surface; in the oxidizing flame on charcoal, it evolves abundant arsenical fumes, rotates, and ultimately leaves a globule of metallic copper, malleable, but still retaining some arsenic: on cupelling this button of copper with lead, a minute globule of silver is obtained; with fluxes, it gives the reactions of copper only.

A qualitative examination by the wet way confirmed these results, and showed the presence of copper, arsenic, and silver without other admixture. Sulphur and iron were especially tested for, but not found present. In one specimen a trace of lead was detected, which appeared evidently to be an accidental impurity.

On quantitatively analysing the mineral, the following results were obtained :—

	A.	B.	C.	D.
Copper . . .	88·35	88·07	88·11	88·02
Silver . . .	0·38	0·24	0·08	0·42
Arsenic . . .	11·27	11·69	11·81	11·56
	<u>100·00</u>	<u>100·00</u>	<u>100·00</u>	<u>100·00</u>

In the Analysis A the arsenic was determined from the amount of arseniate of magnesia and ammonia, previously dried at 212°F., obtained by dissolving 11·01 grs. of the mineral in nitrohydrochloric acid, supersaturating with ammonia, and afterwards adding chloride of magnesium along with chloride of ammonium. The silver was determined by cupellation, and the copper reckoned as loss.

In the Analyses B, C, and D, the silver was, as before, estimated by cupellation with lead; but the arsenic, on the contrary, was reckoned as loss after determining the copper present by the blowpipe as follows :—

From 1 to 2 grains of the mineral were fused upon charcoal in reducing flame along with a very small amount of borax-glass and a previously tared globule of pure gold, the globule of gold being from 3 to 4 grains in weight: when perfectly fused, the united globule was separated from the adherent borax-glass by dipping it into water after solidification, but whilst still hot, This globule was then placed in a charcoal bore, and, after fusion, very carefully treated with a gentle oxidating flame as long as arsenical flames were evolved, and until the instant when the greenish appearance peculiar to melted copper when pure was observed, when it was found to have eliminated all the arsenic, and there remained a perfectly malleable red metallic button of an alloy of gold and copper, the weight of which, less that of the globule of pure gold added, gave the amount of copper and silver present in the ore; and deducting from this the proportionate quantity of silver found by cupelling with lead a separate portion of the mineral, the quantity of copper present was ascertained.

The results obtained by this method are very accurate; and it can frequently be used with advantage in determining copper in compounds of this metal with arsenic when free from sulphur. Iron and gangue do not affect its accuracy, as they are slagged off easily in the fusion with borax-glass. In the assay of malachites and oxides or silicates of copper, the copper present can easily be brought to the state of an arsenide by a previous operation, and then determined as above.

The results above stated leave no doubt that the constitution of this mineral may be represented by the formula Cu^{18}As ,

which by calculation would afford the following per-centage composition :—

Copper	.	.	.	88·37
Arsenic	.	.	.	11·63
				<u>100·00</u>

closely approximating to the results obtained; the mean of which, if the silver be added to the copper, would give—

Copper	.	.	.	88·41
Arsenic	.	.	.	11·59
				<u>100·00</u>

It is interesting to note the occurrence of the three compounds of copper with arsenic in Chile, although, as far as I am aware, no two of these are found at the same locality; but all three occur in veins of the same geological age. Their chemical composition is as follows :—

Domeykite, $\text{Cu}^6 \text{As}$	{	Copper	.	.	71·64
		Arsenic	.	.	28·36
					<u>100·00</u>
Algodonite, $\text{Cu}^{12} \text{As}$	{	Copper	.	.	83·66
		Arsenic	.	.	16·34
					<u>100·00</u>
Darwinite, $\text{Cu}^{18} \text{As}$	{	Copper	.	.	88·37
		Arsenic	.	.	11·63
					<u>100·00</u>

The name Darwinite has been adopted in honour of Darwin, whose admirable geological examination of this part of South America is so well known as to require no comment.

LVIII. *On the Form assumed by a Fluid Shell revolving freely within a Hollow Spheroid.* By G. R. DAHLANDER*.

LET there be a shell of solid matter whose internal and external surfaces are both spheroids having the same axis and centre. Within this suppose there to be a hollow fluid shell having the same centre, but not being in contact with the internal surface of the other; and suppose it to revolve about the common axis with a uniform velocity: then, under the joint in-

* Communicated by the Author.

fluence of the force of attraction and its own centrifugal force, it will under certain circumstances assume a position of equilibrium in which its bounding surfaces will be two spheres concentric with the spheroidal surface of the external solid shell.

In order to show this, let the axis of rotation be the axis of z of a system of rectangular coordinates, whose origin is in the common centre of the spheres and spheroids. Let X, Y, Z be the component parts parallel to the axes x, y, z of the attraction which the hollow spheroid and the fluid shell exert on a particle of the latter. The component parts of the attraction of the outer spheroid may be denoted by

$$-Mx, \quad -My, \quad -Nz,$$

and those of the inner spheroid by

$$-M'x, \quad -M'y, \quad -N'z,$$

where $M, N, M',$ and N' are quantities independent of x, y, z . Let ρ' be the density of the fluid. Then the attraction which the fluid shell exerts on the point x, y, z , has for components

$$\begin{aligned} & -\frac{4}{3}\pi\rho'fx + \frac{4}{3}\pi\rho'f\frac{r^3x}{\sqrt{(x^2+y^2+z^2)^3}}, \\ & -\frac{4}{3}\pi\rho'fy + \frac{4}{3}\pi\rho'f\frac{r^3y}{\sqrt{(x^2+y^2+z^2)^3}}, \\ & -\frac{4}{3}\pi\rho'fz + \frac{4}{3}\pi\rho'f\frac{r^3z}{\sqrt{(x^2+y^2+z^2)^3}}, \end{aligned}$$

where r is the radius of the inner sphere.

The component parts of the attraction of the hollow spheroid and the fluid shell will consequently be

$$\left. \begin{aligned} X &= \left(-M + M' - \frac{4}{3}\pi\rho'f + \frac{4}{3}\pi\rho'f\frac{r^3}{\sqrt{(x^2+y^2+z^2)^3}} \right) x, \\ Y &= \left(-M + M' - \frac{4}{3}\pi\rho'f + \frac{4}{3}\pi\rho'f\frac{r^3}{\sqrt{(x^2+y^2+z^2)^3}} \right) y, \\ Z &= \left(-N + N' - \frac{4}{3}\pi\rho'f + \frac{4}{3}\pi\rho'f\frac{r^3}{\sqrt{(x^2+y^2+z^2)^3}} \right) z. \end{aligned} \right\} \quad (1)$$

The differential equation for the *surfaces de niveau*, when w denotes the angular velocity, is therefore

$$\begin{aligned} & \left(-M + M' - \frac{4}{3}\pi\rho'f + \frac{4}{3}\pi\rho'f\frac{r^3}{\sqrt{(x^2+y^2+z^2)^3}} + w^2 \right) (xdx + ydy) \\ & + \left(-N + N' - \frac{4}{3}\pi\rho'f + \frac{4}{3}\pi\rho'f\frac{r^3}{\sqrt{(x^2+y^2+z^2)^3}} \right) zdz = 0; \quad (2) \end{aligned}$$

for the innermost bounding surface is $x^2 + y^2 + z^2 = r^2$, and $x dx + y dy + z dz = 0$. Hence we find that

$$w^2 - M + M' = -N + N' \quad . \quad . \quad . \quad (3)$$

is a conditional equation, and equation (2) will therefore be

$$\left(-N + N' - \frac{4}{3} \pi \rho' f + \frac{4}{3} \pi \rho f \frac{r^3}{\sqrt{(x^2 + y^2 + z^2)^3}} \right) (x dx + y dy + z dz) = 0. \quad (4)$$

The *surfaces de niveau* are therefore all spherical. If their radius be denoted by r' , we get

$$x dx + y dy + z dz = r' dr'; \quad . \quad . \quad . \quad (5)$$

and if the pressure be denoted by p , we get

$$\frac{dp}{\rho'} = \left(-N + N' - \frac{4}{3} \pi \rho' f + \frac{4}{3} \pi \rho f \frac{r'^3}{r^3} \right) r' dr'. \quad . \quad (6)$$

By integrating this we find

$$\frac{p}{\rho'} = (-N + N') \frac{r'^2}{2} - \frac{2}{3} \pi \rho' f r'^2 - \frac{4}{3} \pi \rho f \frac{r'^3}{r^3} + C.$$

But $r' = r$ when $p = 0$, whence

$$\frac{p}{\rho'} = (-N + N') \frac{(r' - r^2)}{2} - \frac{2}{3} \pi \rho' f (r'^2 - r^2) - \frac{4}{3} \pi \rho' f r^2 \left(\frac{r'}{r} - 1 \right). \quad (7)$$

Equation (7) gives the pressure which takes place in different parts of the fluid. If r' be taken as the radius R of the outer sphere, then p must be $= 0$. Thus we get the conditional equation,

$$(-N + N') \frac{(R^2 - r^2)}{2} - \frac{2}{3} \pi \rho' f (R^2 - r^2) + \frac{4}{3} \pi \rho' f r^2 \left(1 - \frac{r}{R} \right) = 0. \quad (8)$$

Equations (3) and (8) determine the relation which must exist between the quantities therein occurring, in order that equilibrium may be possible under the given conditions. If $\frac{r}{R}$ be denoted by k , then equation (8) becomes

$$\left(\frac{-N + N'}{2} - \frac{2}{3} \pi \rho' f \right) (1 + k) + \frac{4}{3} \pi \rho' f k^2 = 0. \quad . \quad (9)$$

Hence we find that if, under the given conditions as to the dimensions of the spheroid and the density of the fluid, equilibrium is possible in one case, there are an infinite number of other cases in which equilibrium can also take place, provided r and R may vary but k is supposed to be constant.

Equation (9) is of the second degree with regard to k ; but

it is easy to see that it can only have one positive root, because the first term must be negative.

Let us particularly consider the case in which the outer bounding surface of the spheroid is spherical, and the inner surface is oblate. Then

$$\left. \begin{aligned} N &= M = \frac{4}{3} \pi \rho f, \\ M' &= 2 \pi \rho f \frac{(1 + \lambda^2)}{\lambda^3} \left(\arctan \lambda - \frac{\lambda}{1 + \lambda^2} \right), \\ N' &= 4 \pi \rho f \frac{(1 + \lambda^2)}{\lambda^3} (\lambda - \arctan \lambda), \end{aligned} \right\} \quad (10)$$

where ρ denotes the density of the spheroid. Putting

$$\frac{w^2}{2 \pi \rho f} = E, \text{ and } \frac{\rho}{\rho'} = m,$$

equations (3) and (9) become

$$E = m \left(\frac{3 + 2\lambda^2}{\lambda^2} - \frac{3(1 + \lambda^2)}{\lambda^3} \arctan \lambda \right), \quad (11)$$

$$\frac{k^2}{1 + k} = \frac{m + 1}{2} - \frac{3}{2} m \frac{(1 + \lambda^2)}{\lambda^3} (\lambda - \arctan \lambda) = \frac{1}{2} (1 - E). \quad (12)$$

Equations (11) and (12) determine the relations which must exist between λ , ϵ , m , and k , in order that equilibrium may result. If the right-hand member of (12) be denoted by n , we get

$$\frac{k^2}{1 + k} = n.$$

This equation, if equilibrium be possible, must give a positive fraction as the value of k . If resolved in regard to this quantity, we get, observing that n must necessarily be positive,

$$k = \frac{n}{2} + \sqrt{\frac{n^2}{4} + n},$$

which gives as the condition for $k < 1$,

$$n < \frac{1}{2}, \text{ or } 1 - \frac{3(1 + \lambda^2)}{\lambda^3} (\lambda - \arctan \lambda) < 0. \quad (13)$$

This condition is always fulfilled when $\lambda > 0$. In order that n may be positive,

$$m \left(1 - \frac{3(1 + \lambda^2)}{\lambda^3} (\lambda - \arctan \lambda) \right) + 1 > 0, \text{ or } \epsilon < 1. \quad (14)$$

This inequality determines the greatest value m can have when

λ has a given value. Hence it appears that when λ is given, we can always find such positive values for m , that the condition given by the inequality may be fulfilled. Equation (11) in that case always gives positive values for ϵ when λ and m are determined.

Suppose, for example, $\lambda=1$, then the condition (14) will be

$$-0.2876m + 1 > 0, \text{ or } m < \frac{1}{0.2876}.$$

Suppose, for instance, $m=1$, or that the densities of the solid spheroid and of the fluid sphere were equal, then we should get

$$\frac{k^2}{1+k} = 0.3562, \text{ whence } k = 0.801.$$

The inner radius of the hollow sphere ought therefore to be about four-fifths of the outer one. Lastly, we find from (11) that

$$\epsilon = 0.2876.$$

There is another class of forms of equilibrium which may be assumed by fluids revolving in a hollow spheroid, which we will only mention here, as being of comparatively less interest. A heavy homogeneous fluid, revolving in the cavity of the hollow spheroid, may assume a spheroidal figure of equilibrium. In fact, when M, M', N , and N' have the same signification as before, and $-M''x, -M''y$, and $-N''z$ denote the component parts of the attraction of the fluid spheroid, then the equation of equilibrium will be

$$(-M + M' - M'' + w^2)(x dx + y dy) + (-N + N' - N'')z dz = 0, \quad (15)$$

where $M, M', M'', N, N', N'', w$ are independent of x, y, z . If the equation of the fluid surface be supposed to be

$$\frac{x^2 + y^2}{a^2} + \frac{z^2}{a^2(1 + \lambda^2)} = 1,$$

then the condition of equilibrium will be

$$1 + \lambda^2 = \frac{-M + M' - M'' + w^2}{-N + N' - N''}. \quad \dots \quad (16)$$

By putting the values for M, M', M'', N, N', N'' in equation (16), we find that equilibrium is possible under an infinite number of different conditions with regard to the form and density of the spheroids, and the velocity of rotation of the fluid mass.

Gotlibenburgh, October 24, 1860.

LIX. *A Theory of Galvanic Force.* By Professor CHALLIS, F.R.S.*

HAVING argued in previous communications that the laws of Light, Heat, Gravity, and Electricity may be consequences of the dynamic action of an elastic fluid medium, I proceed now to apply the principles of the same theory to the facts of galvanism. As it is hardly possible that so comprehensive a theory could escape the contradiction of facts, unless it were well founded, every extension of it to new classes of facts without meeting with contradictions, and without the necessity of new hypotheses, adds greatly to the evidence for its truth. In art. 18 of the "Theory of Electric Force," given in the October Number, I have shown that electrical attractions and repulsions might be explained consistently with hydrodynamic action by the hypothesis of ætherial *currents*. Now it is this very hypothesis which is required for explaining by the same kind of action the laws of galvanism. It must be assumed, on the hydrodynamical theory, that there exist in the neighbourhood of the earth *steady* ætherial currents, which within considerable spaces may be considered to be of uniform velocity and density. By the solution of Problem II., in the "Theory of Attractive Forces" contained in the Philosophical Magazine for November 1859, it appears that these currents would produce no motion of translation of small solid spheres, such as the ultimate atoms of bodies are assumed to be. But the same currents might, under the circumstances stated in art. 18 above referred to, give rise to steady *secondary* currents, which, not being uniform as to velocity and density, might be capable of acting dynamically to a sensible amount on ultimate atoms. That the hypothesis of steady primary currents is also in accordance with *magnetic* phenomena, I hope to show hereafter, and at the same time to consider what may be their origin.

1. One of the primary facts of galvanism is its intimate connexion with electricity. The following illustration may help to form a distinct idea of the explanation which the hydrodynamical theory gives of this fact. Conceive any solid substance to be suspended by attachment at one of its points to an immoveable body, and to be acted upon by gravity. Then assuming it to consist of discrete atoms all equally urged by the force of gravity, the lower superficial atoms must be kept in equilibrium by an excess of molecular attraction above molecular repulsion, and the higher by an excess of molecular repulsion above molecular attraction. Also the force of gravity on any atom in the interior of the body must be just counteracted by the resultant of forces emanating from the atoms. But the

* Communicated by the Author.

internal atomic actions are equal and opposite at any point of the interior if the density of the body be uniform. Hence, besides the rapid gradations of density near the boundary, which give rise to the superficial attractions and repulsions, there must be a much slower gradation of density throughout the interior of the substance; and as the atomic and molecular repulsions are much more affected by difference of density than the molecular attractions, the force of gravity is counteracted by repulsions, and the density consequently *decreases* from the *lower* to the *upper* part of the body. Now it is clear that in this example the deviations of the superficial atoms, and those of the interior atoms, from their undisturbed state, are mutually dependent, and that all are connected with the molecular action at the point of attachment; for if the attachment were suddenly cut off, the atoms would immediately return to the undisturbed state, and would all equally obey the force of gravity. While in this instance the atoms are all in disturbed positions, because all are acted upon by the force of gravity, the counteracting atomic and molecular forces consequent upon this state of disturbance, must evidently have a single resultant at the point of attachment. It is therefore conceivable that conversely a superficial disturbance at a single point, or at several points, may extend its influence over all the atoms of a body. This, as a matter of experience, appears to be the case when *two dissimilar substances, conductors of electricity, are in contact.*

2. The definition of physical contact is, that the superficial atoms of each body are acted upon at the points of contact by the molecular forces of the other; not that the atoms themselves come into contact, this being prevented by the atomic repulsion, which is the most energetic of all the physical forces. When two dissimilar substances are in contact, the superficial atoms of each at and near the points of contact must, in general, on account of the dissimilarity, be put by the molecular forces into positions of disturbance; that is, according to the theory of electricity which I have proposed, the two substances are in an *electric* state. If they are good conductors of electricity, the local disturbance extends throughout their superficial atoms, and the substances are in a state of *induced* electricity, which would immediately disappear upon separating them. Under these circumstances, from what is argued above, there would be a gradation of density throughout the interior of each, and thus the conditions for generating *secondary* currents, stated in art. 18 of the "Theory of Electricity," would be fulfilled. The intensity of these currents will depend on the amount of disturbance at the points of contact; and this again must depend on the constitutions and qualities of the two substances. It might

be anticipated that the action between a solid and a fluid would be considerable, on account of the dissimilarity of their superficial molecular conditions. If a chemical affinity exists between the solid and the fluid, the disturbance may be expected to be still greater, because such affinity exhibits itself by a tendency in the superficial action so far to disturb the atoms as to put them into new relations. The action between two gases (as oxygen and hydrogen) which tend to combine might produce sensible secondary currents, while no such effect would follow from the contact of two gases (as oxygen and nitrogen) which have no chemical affinity. These are, in short, facts of experience which are at once seen to be in accordance with the theory, if it may be assumed that *the secondary ætherial currents of the theory are the same as the galvanic currents of experiment.*

On this assumption the theory explains the fact that substances which generate galvanic currents are good conductors of electricity, and exhibits the connexion of the development of electricity with the generation of the currents.

3. It appears from experiment that different substances have different powers of transmitting galvanic currents. There are conductors and non-conductors of galvanism, as of electricity; and, generally, the substances which conduct electricity also conduct galvanic currents. Air is a non-conductor as well of galvanic currents as of electricity. The ratio of the two conductive powers does not appear to be the same for different substances. It does not belong to the present theory to inquire into the reasons for these facts, which are mentioned here only because they bear upon the subsequent explanation of the hydrodynamical theory.

For instance, it may be asked, if the origin of galvanic currents be such as the theory indicates, why do not the same attractions and repulsions of light bodies take place in the neighbourhood of the voltaic battery as in the neighbourhood of a body whose electricity is excited by friction? To this I reply that the induced electricity at any point of the voltaic battery is very much feebler than the electricity induced when the original disturbance of the superficial atoms is caused by friction, and the currents due to a feeble induction are proportionally feeble. Any perceptible motive power of *partial* currents, acting *through* the air, may be prevented by the non-conductiveness of the air itself, and the insulation of the battery. But when the separate streams excited at a great number of points (as in a large battery consisting of numerous cells) are, as it were, concentrated in an *electrode* of fine wire, considerable motive power acts through the air, as is evident by the known action of such electrodes on each other when, being arranged so as to be near

and parallel to each other, they are at the same time free to move.

4. The question as to whether the galvanic currents owe their origin to the contact of dissimilar substances, or to chemical action, has been much discussed on experimental grounds. Looking at it from the point of view of the hydrodynamical theory, the conclusions that will be come to are, that the currents are due to the mutual molecular actions of dissimilar substances in contact; that chemical affinity between the substances augments the molecular actions, and consequently the strength of the currents; and that chemical combination, or analysis, is not necessary for the generation of the currents, but may be a consequence of them. I consider the theory to be in complete accordance with the two following inferences, which occur in an exposition of the experimental evidence bearing on this point by Mr. Gassiot (Philosophical Transactions, 1844, p. 39). "Elective affinity is greatly concerned in the *antecedent* action, of which chemical combinations, when the circuit is closed, are the *consequence*" (§ (26)). "The higher the chemical affinities used, the greater was the evidence of the development of tension" (*Note* at the end of the Paper).

5. The explanation which the hydrodynamical theory gives of the chemical action *consequent* upon the generation of the currents, will be best stated after a more particular consideration of the nature of the currents, considered as instances of steady fluid motion. The origin of the movement being at the battery, and the current flowing from the right hand through the battery to the left hand, the fluid will be *impelled* on the left hand, and will be *drawn* on the right hand. It will enter the conductor at one pole, and issue out at the other; and by this circumstance the two poles will be permanently distinguished. The direction of the stream depends on the order of the elements, the stream resulting from a *difference* of action of the elements; and if the order be not the same throughout the battery, opposing streams will be generated. Now it does not appear possible for steady currents to give rise to forces adequate to produce chemical analysis, except by considerable changes of velocity within small spaces. By hydrodynamics, the variation of pressure dp in an elementary portion ds of a line s drawn in the direction in which the stream flows, is proportional to $-VdV$, dV being the corresponding variation of the velocity V of the stream. Hence an increment of velocity in the direction of the stream is accompanied by a decrement of pressure, and consequently an accelerative force, in the same direction; and, on the contrary, a decrement of the velocity in the direction of the stream produces an accelerative force in the opposite direc-

tion,—the forces, in each case, being proportional to the rate of change of pressure.

6. In the “Mathematical Theory of Attractive Forces,” given in the Philosophical Magazine for November 1859, I have shown by the solution of Problems II. and III., that when a small spherical body in a fixed position is submitted to the action of a *uniform* stream, the pressures on opposite hemispheres are equal, and consequently that the stream would impress no velocity on the sphere supposing it free to move, nor alter any velocity which it may have otherwise acquired. The effect, however, is different if the stream, though steady, be not uniform, that is, if there be change of velocity, and consequently change of density, from point to point in its course. In that case, the mere *statical* effect of change of density would produce an accelerative force of the body in the direction of the decrement of density. The total effect will, however, be due both to the *motion* of the fluid and to the variation of its density. I shall not now attempt to solve the problem of calculating this effect mathematically, as the following general considerations may suffice for the present purpose. It has already been stated, as a result of mathematical reasoning, that when a small solid sphere moves uniformly in an elastic medium, the pressures on the preceding and following hemispherical surfaces are equal. Hence there is no tendency to acceleration or retardation of the sphere; and the motion of the fluid being the same in successive instants, the total momentum of the fluid and sphere remains the same in successive instants. This permanence of the momentum in cases where no external force acts, may be regarded as a general law of the mutual action between a solid and a fluid. Accordingly, since in the case before us the spherical body is accelerated by the fluid, it will impress at each instant on the fluid in the contrary direction by the reaction on its surface, a momentum equal to that which it receives. But the reactions of spheres of different radii are, *ceteris paribus*, proportional to their surfaces, that is, to the squares of the radii. Hence the instantaneous increments of momentum of the spheres are in the same proportion, and the accelerations are therefore in the inverse proportion of the radii. From this reasoning it follows that when the atoms of any substance are submitted to the action of a steady ætherial current of variable density and velocity, they are accelerated in the direction of the decrement of density in proportion to some function of the decrement, and the less are more accelerated than the greater.

If the decrement of density be in a direction contrary to that of the stream, the acceleration of the atoms still takes place in the direction of the decrement.

7. Let us now consider the process of generation of ætherial currents, and their dynamical action, in the experiment of the *galvanic battery*. It will suffice to take the simple case of a plate of zinc and a plate of copper immersed in an acid solution. The chemical action between the fluid and zinc may be such as to produce analysis of the fluid anterior to any galvanic current, while no such action takes place at the surface of the copper. This *difference* between the relations of the metals to the fluid is an essential condition of the generation of a current. The chemical affinity between the acid solution and the zinc, causes a change of position of the superficial atoms of the latter, and puts it into an *electric* state, the action of the fluid taking the place of friction in the phænomena of ordinary electricity. If the zinc plate were alone immersed in the fluid, all the points of its surface would be in the same electric state; but being in the presence of the copper, the latter is electrified *inductively*, and reacts upon the zinc. Thus *both* metals are put into a state in which a gradation of interior density exists, and consequently, according to our theory, each tends to generate secondary currents. Also the tendency is in the same direction for both, because the adjacent parts are oppositely electrified. No current, however, flows unless a continuous route is provided by means of a metallic connexion between the zinc and the copper. Till this is done the current is driven back by the insulation of the battery and the non-conductiveness of the air. But when the circuit is complete, the internal atomic conditions of the zinc and copper, which originate the secondary current, maintain its dynamic action. So far as relates to the action of the current, the case is exactly analogous to that of electrical *attraction*, considered in the "Theory of Electricity." As the attraction towards each other of the electrified bodies was due to a steady current, having a position of maximum velocity, and therefore of minimum density between them, so in this instance there will, for like reasons, be a position of minimum density of the æther between the zinc and the copper. Consequently there will be a decrement of density more or less rapid from the zinc towards this position; and if it be assumed that the current flows in that direction, from what is argued in the preceding paragraph, its dynamic action, being more energetic on the atoms of hydrogen than on the larger atoms of oxygen, will urge the former towards the copper. After accumulating on the surface of the copper, the hydrogen will rise in the fluid in the form of bubbles. In the mean time the oxygen set free obeys the law of its affinity for zinc, and combines with it more copiously than it did before the flow of the current. In this way the theory accounts for *electrolysis* by the dynamic action of an ætherial

current. I have assumed the atoms of hydrogen to be smaller than those of oxygen in accordance with a theoretical result obtained in the Philosophical Magazine for February 1860. If this be a true explanation, the theory decides that the flow of the galvanic current is *from the zinc to the copper*, that is, from the *positive* to the *negative* pole of the battery.

8. In the usual experiment for decomposing water, the terminals being of platinum wire, there is no direct action between the metal and the fluid; but as one wire is connected with the positive pole of the battery, and the other with the negative, they must have opposite electricities, and will also act upon each other inductively. Hence, by the same reasoning as before, an ætherial current is generated, which may be capable of separating the hydrogen from the oxygen by impelling the atoms of hydrogen more energetically than those of oxygen. In this case the oxygen, not having an affinity for platinum, rises in bubbles at the *positive* terminal, while the hydrogen is driven off to the *negative* terminal. As there must be a decrement of density at the negative terminal towards the position of minimum density between the two, it is possible there may be a decomposition of the water at this terminal also, but in less degree than at the positive, because the decrement of density is in the direction *contrary* to that of the stream. If there be such decomposition, all the oxygen liberated may unite with hydrogen sent from the other terminal, and thus hydrogen alone will rise from the *negative* terminal, as is known to be the fact from experience. This last remark applies also to the case considered in the preceding paragraph, if there should be any decomposition, by the agency of the current, at the surface of the copper.

9. It will be seen from the above explanations, that the generation of a galvanic current without actual analysis of the fluid, the possibility of which appears to be proved by the *water-battery*, is consistent with the theory.

10. The investigation of the motion of the ætherial current along an *electrode* of fine wire is a very difficult hydrodynamical problem, the solution of which I shall not attempt to give at present *ab initio*. But admitting the existence of such currents, and that they are *steady*, it is not difficult to state what must be their chief characteristics. In the first place, if not confined within limits by non-conducting substances, they must exert an influence at considerable distances from the electrode. Hydrodynamically it would be impossible for an unconfined stream to flow along the electrode without producing motion in the surrounding fluid. The velocity of the æther is greater within the wire than without, because the wire is the effective channel of the stream, and the motion of the surrounding fluid is the result

of lateral action. The velocity within the wire is also increased by the contraction of the channel by the occupation of space by the atoms. Now as the axis of the electrode thus becomes a line of maximum velocity, and therefore of minimum density, there must be a tendency of the contiguous fluid to flow towards this axis. But if this motion actually took place, the stream would immediately be stopped. Hence this tendency must be counteracted by a uniform circular movement kept up by the accelerative force due to the decrement of density towards the axis. Thus the resulting motion will be in *spiral lines about the electrode*. This conclusion will be put to a severe test when we come to consider the mutual action of galvanic currents and magnets.

11. It is readily seen from this theory why the current flows as soon as two wires connected with the poles of a battery are put in contact; for by the action of the battery the fluid is *impelled* along one wire, and *drawn* along the other, and the *circulation* of the stream results from the junction. Simple contact suffices to produce the effect, apparently because it provides an axis for the spiral movement, which, as explained above, is necessary for maintaining the current. It is not so easily seen why there is no current unless the poles are brought into connexion. When the ends of the wires are in air, it is sufficient to say that the air, being a non-conductor, stops the flow of the current, the explanation of the non-conductive property being not at present under consideration. But the experiments of Mr. Gassiot have proved (Phil. Trans. vol. cxlix. part 1. p. 156) that a *perfect vacuum is a non-conductor*. The explanation of this fact must be drawn from the laws of movement of the ætherial fluid. On this point I have the following considerations to offer. Assuming that galvanic currents are identical with ætherial currents, experience shows that the conductive property of a substance allows an ætherial stream to permeate freely through its whole interior; and, supposing it to be insulated, that its bounding surface contains the stream within limits, just as a closed channel of any shape contains a stream of water. In this manner a metallic wire is a channel for the ætherial current; but in this case, if the insulation be not perfect, on account of the narrowness of the channel, the surrounding æther is perceptibly acted upon, so as to make a spiral movement necessary, as above stated, for the maintenance of the current. The spiral motion may be conceived to originate where the stream issues from a wider space into the electrode, just as the same phenomenon occurs when water issues from a vessel through an orifice; also, where the stream issues from the electrode into a wider space, a like movement may be produced. In both cases, a rapid change

of the transverse dimensions of the channel is a necessary condition, the stream being throughout confined within limits. But when the extremities of the electrodes are in vacuum, the continuity of the channel is broken; and as from hydrodynamical principles it may be shown that the æther is incapable, by the mutual action of its parts, of maintaining a spiral movement, this movement ceases, first at the extremities of the electrodes, then through their whole lengths, and the current is consequently stopped.

12. It appears from experiment that discharges of great lengths may be passed from the extremity of one electrode to that of the other, within glass tubes of small bore, wholly or nearly exhausted. This may be explained on the principles of the theory by saying that the form and non-conductive quality of the tube are favourable to the maintenance of the spiral movement.

13. The current is maintained when the electrodes are connected with the earth instead of being connected with each other, simply because the earth is a conductor which may be considered to be of unlimited extent. On this account the *drain* which takes place at the extremity of one of the electrodes cannot produce an exhaustion of æther sufficient to cause a revulsion of the stream, nor can the *flow* at the extremity of the other electrode cause a repletion which would send back the stream.

14. It is stated in Davy's 'Elements of Chemical Philosophy' (vol. i. p. 152), that with the same battery an *electric* discharge took place in air of ordinary density between charcoal points distant from each other about one-thirtieth of an inch, and in rarefied air which supported one-fourth of an inch of mercury, the sparks passed through nearly half an inch: also that in the former case the constant or galvanic discharge took place in the heated air through a space equal at least to four inches, and in the latter through a space of six or seven inches. These facts prove experimentally, not only that air resists the passage both of electricity and of the galvanic current in greater degree as its density is greater, but seem to show also that the galvanic discharge may go on while the battery is in a state of electric tension. This inference is consistent with the hydrodynamical theory.

15. The theory distinctly separates the electric from the galvanic discharges, the former being abrupt and violent streams produced by the sudden return of *superficial* atoms in disturbed positions to their natural relative state, and the latter being the *continuous* effect of a disturbed state of the *interior* atoms. These conditions of the superficial and the interior atoms are, however, so intimately connected, that when the galvanic current

is flowing in a closed circuit, it must be supposed that very minute and rapid electric discharges are taking place at the same time.

16. The *light* and *heat* accompanying electric and galvanic discharges may, according to the Undulatory Theories of Light and Heat which I have proposed, be ascribed to vibrations of the atoms of æriform or solid bodies, excited by the dynamic action of ætherial currents. When the electrodes terminate in charcoal points, the dynamic action is increased both by the condensation of the streams due to the pointed form, and, on approaching the points, by the impulse given to the streams in consequence of the opposite states of electricity of the terminals.

It seems to be a sufficient explanation of the *voltaic arc*, to say that the air between the poles, being heated by the galvanic discharge, continually rises, and that consequently the path of least resistance, which depends only on the density of the air, is not the straight line joining the poles, but a course above this.

17. The *coloured light* in the tubes with which Mr. Gassiot experimented seems to be caused by vibrations of some gaseous substance enclosed within them, the atoms of which, it being of extreme rarity, may be made to perform large excursions by the dynamic action of the current, and reciprocally may produce agitations of the æther rendered sensible in the form of light. If the gas were of considerable density, its elastic force would resist the action of the stream, and the excursions of its atoms might consequently be too small to produce a sensible amount of light. The *stratification* may be due to the *nodes* and *loops*, that is, positions of minimum and maximum vibration, which are always consequent upon exciting vibrations of elastic gases in tubes. The colour is probably to be ascribed to the particular quality of the gas. The *glow* at the terminals appears to have a different origin from that of the stratification, and may possibly be due to the agitations of the æther, caused by the sudden changes of the stream on its passing out of, or entering into, a terminal.

18. Different dynamic effects of the motions of the æther may *co-exist* without mutual disturbance, if the motions be *vibratory*; but the dynamic effects of steady *currents* interfere with each other. This known result from hydrodynamical principles at once explains the *disturbance of a galvanic current by a magnet*, supposing the magnetic influence to be due to steady ætherial currents.

19. If two electrodes be placed near each other and parallel, and galvanic currents pass along them in the *same* direction, they are *attracted* towards each other. The explanation of this fact by the theory is, that the contiguity of the electrodes de-

stroys the symmetrical arrangement of the density of the æther about them; and since when they are distant from each other the density decreases towards their axes, on their being brought near, the space between them becomes a position of minimum density, towards which they are consequently attracted. If the currents pass in *opposite* directions, by their interference they produce an excess of density between the electrodes, which are consequently *repelled*, as is known experimentally. The interference of the transverse motions would produce the contrary effects. It seems, therefore, that the observed attractions and repulsions are due to the excess of the longitudinal above the transverse action of the currents.

20. It is scarcely necessary to say that the velocity of a galvanic current in an electrode is very different from the velocity with which the commencement of the current, or any *interruption* of it, as in the use of the electric telegraph, would be propagated along the electrode through space. The latter velocity, on hydrodynamical principles, would be comparable with the velocity of transmission of light through solid substances, and would probably exceed the velocity with which light would traverse a substance of the same density as the electrode.

The foregoing explanations seem to justify the conclusion that the facts of galvanism may with much probability be referred to the dynamical action of the ætherial fluid, and consequently be included within the proposed hydrodynamical theory of the physical forces.

Cambridge Observatory,
November 16, 1860.

LX. *Note on the Inductive Spark.* By Dr. P. L. RIJKE, Professor of Natural Philosophy in the University of Leyden*.

1. **A**BOUT five years ago Vicomte du Moncel first called attention to the fact† that the inductive spark obtained from Ruhmkorff's apparatus differs materially from the spark obtained on discharging, by ordinary means, a common electrifying machine or a Leyden jar. In the latter case the spark is homogeneous, and consists of a simple point of light, while the spark obtained from Ruhmkorff's apparatus is composed of two parts altogether different from each other,—the one being a point of light precisely similar to the ordinary spark, the other a luminous atmosphere that admits of being displaced by means of a

* Communicated by the Author.

† *Comptes Rendus*, vol. xl. p. 313; and *Phil. Mag.* S. 4. vol. ix. p. 546.
Phil. Mag. S. 4. Vol. 20. No. 135. Dec. 1860. 2 G

current of air or gas. Another French philosopher, M. Perrot, afterwards discovered* that the heating power of this luminous atmosphere is much greater than that of the true spark; and at the same time he found the means of completely separating the atmosphere from the rest of the spark. For this purpose M. Perrot opposed to one of the electrodes another of a V-shape, and found that, by suitably regulating the distance of the extremities of the latter from the former, he could establish an atmospheric current which carried the luminous atmosphere towards that branch of the V-shaped electrode which was more remote from the opposite pole. Under these circumstances the luminous atmosphere appeared only at this latter branch, the other branch receiving the ordinary spark. About the same time M. du Moncel submitted the inductive spark to the action of powerful electro-magnets, and discovered that while the ordinary spark is entirely unaffected by magnetic force, this luminous atmosphere appears affected in precisely the same manner as the voltaic arc under similar circumstances.

2. When an ordinary Ruhmkorff's apparatus is made use of, most of the phænomena I am about to mention would require to be observed by means of a microscope. This, however, is not the case when more powerful apparatus is employed. I have recently repeated all these experiments with a machine lately constructed by M. Ruhmkorff, and which produces sparks 31 centims. long. With this apparatus the phænomena in question are produced on a scale of such magnitude as to be perfectly visible to the naked eye.

3. M. du Moncel has endeavoured, as is well known, to explain these phænomena by means of a theory which he has propounded in several communications to the Academy of Sciences†. Unfortunately, however, this theory has not, I believe, obtained the general assent of physicists; and the same may be said of other attempted explanations that have been advanced by various other distinguished philosophers. There is, therefore, a want here which I am about to satisfy by means of a new theory, which will be seen, however, to bear some resemblance to that of M. van der Willigen‡.

4. If we inquire what is the electric state of the inductive wire at the moment before its discharge, it seems to me that but one answer can be given:—the middle portion is in its natural state, and the two extremities are charged, the one with positive, and

* *Comptes Rendus*, vol. xlix. p. 175.

† This theory is also advocated in a work entitled "Researches on the Non-homogeneity of the Electric Spark," by Vicomte du Moncel. Paris, 1860.

‡ Poggendorff's *Annalen*, vol. xcviii. p. 494.

the other with negative electricity*. As to the intermediate parts, they are also electrified; and although we are unacquainted with the precise law that connects the degree of electric tension with the distance from the middle point, we know at least that these magnitudes increase together. It is, then, evident that when the inductive wire is discharged, the electric charges of the two extremities first unite, and the charges from the parts nearer and nearer the centre follow in succession. Now the fluids contained in the two extremities obviously unite in the form of an ordinary spark; but the same cannot be the case with the charges of the parts nearer the centre. In fact the experiments of Mr. Wheatstone† show that when two electric fluids, before uniting, have to traverse a metallic wire $\frac{1}{15}$ th of an inch in diameter and half a mile long, the duration of the spark is greatly increased, even to a degree that cannot be accounted for by the mere time that electricity would require to traverse half a mile of such wire, which proves that this prolonged duration of the spark is to be attributed to that particular property of matter that physicists have agreed to call electric resistance. Now if a wire of $\frac{1}{15}$ th of an inch diameter, and only half a mile long, sensibly increases the duration of the spark, what must be the effect of the secondary wire of Ruhmkorff's apparatus, the resistance of which is much more considerable? We can form some estimate of it by the time taken, according to the experiments of M. W. Weber‡, by an electric battery to discharge itself when the current has to traverse a wet hempen cord. It is therefore evident, according to my view of the subject, that the electric spark ought to have some duration; and we know from an experiment of M. Lissajons§, that when the inductive spark is observed in a mirror which is at the same time shaken by the hand, the luminous atmosphere presents the appearance of an elongated band, of which the true spark occupies the posterior extremity,—thus showing that the atmosphere in question really does endure for some fraction of a second.

5. We know that the appearance of the electric spark is generally changed when the two fluids, before uniting, have undergone any considerable resistance. Its colour is then altered, it becomes tinged with violet and blue, while its illuminating power is much diminished; at the same time its form is changed, its volume augmented, and it acquires the power of inflaming bodies

* In the first machine constructed by Ruhmkorff, free electricity was only found at one extremity of the wire; this, however, was only in consequence of a defect in the insulation, that M. Ruhmkorff has since remedied.

† Phil. Trans. 1834.

‡ *Electrodynamische Maasbestimmungen*, p. 295.

§ *Comptes Rendus*, vol. xlix. p. 1009.

which resist the effect of the ordinary spark: thus, for example, gunpowder can be exploded by the spark of a Leyden jar, if the electric fluid is first passed through a wet cord. Now these are precisely the characteristics that have been recognized as existing in the luminous atmosphere of the inductive spark.

6. According to the theory here advanced, we ought to be able to augment the luminous atmosphere at the expense of the true spark, and, conversely, the spark at the expense of the atmosphere, as follows:—

7. In order to increase the spark at the expense of the luminous atmosphere, all that is necessary is to unite each extremity of the inductive wire to the armatures of a Leyden jar by means of metallic conductors, and to pass the charge through a Henley's discharging rod, which has one extremity of one of its branches communicating with the knob of the jar, and one extremity of its other branch connected with the external coating, the spark passing between the other two ends of its two branches. This, it will be seen, is precisely the experiment of Messrs. Masson and Grove. It is evident that when the two extremities of the secondary wire communicate with the armatures of a Leyden jar, the distribution of the electric fluid in the wire undergoes a very great alteration, since, before the actual passage of the charge, the greater part of the two fluids will have accumulated in the armatures of the jar, whence it must result that, the two fluids having now to traverse conductors offering little resistance, the discharge ought to take the form of an ordinary spark; and this is found to be the case. M. du Moncel has himself acknowledged that, under these circumstances, the luminous atmosphere no longer makes its appearance. My theory, then, not only accounts for this disappearance of the luminous atmosphere, but it also explains why, in the experiment of MM. Masson and Du Moncel, the brilliancy of the spark is so greatly increased.

8. When, on the other hand, we wish to get rid of the point of light and to retain the atmosphere, all we have to do, according to my theory, is to compel the electricity distributed throughout the secondary wire to traverse a path in which it finds considerable resistance. This I effected as follows:—For my electrodes I took two brass wires about 1 millim. in diameter, and 3 or 4 centimetres long, and having fixed them to insulators, I united them to the extremities of the inductive wire by means of two wet hempen cords, about 5 millims. in diameter and 0·7 metre long. The inducing current was then produced by means of two elements of a Bunsen's battery. Under these circumstances the ordinary spark was no longer visible, the luminous atmosphere alone remained, and the whole of it would be displaced by means of a current of air.

9. If the want of homogeneity of the inductive spark be really due to the cause to which I attribute it, then it ought to be possible to discharge ordinary friction electricity so as to exhibit both the point of light and the luminous atmosphere; and the discharge so modified, when submitted to the action of external agents, ought to behave similarly to the spark produced by Ruhmkorff's apparatus. It will easily be understood that I attached great importance to the success of this experiment, which may be considered as the touchstone of my theory. The way I contrived it was as follows:—The conductor of a hydro-electric machine, the grate of which possessed a surface of 9 square decimetres, was put in metallic connexion with the interior of a Leyden jar, the external coating of which presented a surface of 738 square centimetres, and was in communication with the boiler of the machine. The knob of the jar was also connected by means of a wet hempen cord, 5 millims. in diameter and 45 centims. long, with an insulated metallic sphere of 29 centims. in diameter. The two branches of a Henley's discharging rod constituted the two electrodes. The end of one of them was in connexion with the external covering of the jar, that of the other was at the distance of about 10 millims. from the metallic sphere. The other two extremities of the two branches, which were pointed, were about 7 or 8 millims. apart. It is evident that, with this arrangement of the apparatus, the Leyden jar ought to be discharged whenever the tension of the electricity in the sphere attains a certain amount, and that this discharge ought to appear as a luminous spark between the pointed extremities of the discharging rod.

The electricity which thus passes between these two electrodes proceeds from two different sources, viz. from the sphere, and the interior of the jar. The electricity of the sphere having but a short metallic path to traverse, ought to give rise to the appearance of an ordinary spark; while, on the contrary, the fluid accumulated in the jar having to overcome the very considerable resistance of the hempen cord, ought to give rise to the appearance of a luminous atmosphere. Now the discharge actually observed presents precisely this double appearance.

10. On submitting the luminous discharge of this apparatus to the action of an air-current, I obtained precisely the same results as those observed by M. du Moncel in the case of the inductive spark,—the luminous atmosphere being carried away from the point of light, on which the air-current did not seem to exercise the slightest effect. M. Perrot's experiment also succeeded perfectly. Finally, I examined the nature of the action of magnetic force on the discharge, and I found that the luminous atmosphere under the action of an electro-magnet behaved precisely as in the experiments of M. du Moncel.

11. I trust that physicists will admit that we are justified in concluding, from the foregoing facts, *that in the inductive spark the point of light is to be attributed to the recomposition of the electric charges accumulated in the extremities of the secondary wire, while the luminous atmosphere is produced by the electric fluids contained in the parts of the wire nearer to its middle point.*

Leyden, October 31, 1860.

LXI. *On Approximation to the Integrals of Irrational Functions by means of Rational Substitutions.* By CHARLES W. MERRIFIELD, Esq.*

IN his "Meditation on Poncelet's Theorem" in the October Number of this Journal, Mr. Sylvester has done me the honour to mention a theorem of mine upon the subject expressed at the head of this paper. I have now to communicate an extension of that theorem. It is but right to state that this extension has been suggested to me by Mr. Sylvester's remarks on the connexion between his method and the Newtonian system of approximation to the roots of equations.

We had both taken the pure quadratic as our base of operations; but while he took the method of continued fractions to obtain his converging terms, I took (under another shape) the method of successive substitution. I am compelled to differ from him as to the practical advantages of the two methods for the purposes of the computer.

In my paper, which Canon Moseley communicated to the Royal Society this year, I started from the principle that the geometric mean between two quantities is also a geometric mean between their arithmetic and harmonic means, and again between the arithmetic and harmonic means of those means, and so on,—the series of arithmetic means on the one side, and the series of harmonic means on the other, giving a continual approximation to the geometric mean. Now this process is but a particular case of the Newtonian approximation to the roots of an equation: viz., let a be a first approximate solution, obtained by trial, of the equation $fx=0$, and call $f'x$ the differential coefficient of fx ; then a second approximation is $a - \frac{fa}{f'a} = b$; a third approximation will evidently be $b - \frac{fb}{f'b} = c$, and so forth. If we apply this method to the pure equation $x^n=p$, the convergent terms which we obtain are as follows:—

* Communicated by the Author.

$$b = \frac{(n-1)a^n + p}{na^{n-1}},$$

$$c = \frac{(n-1)\{(n-1)a^n + p\}^n + n^n \cdot p \cdot a^{n(n-1)}}{n^2 a^{n-1} \{(n-1)a^n + p\}^{n-1}}, \&c.$$

If we make $n=3$, we have for the fractions converging to the cube root,

$$a, \quad \frac{2a^3+p}{3a^2}, \quad \frac{2(2a^3+p)^3+27pa^6}{9a^2(2a^3+p)^2}, \&c.$$

Applying this to the cube root of 2 by making $a=1, p=2$, we obtain successively,

$$1, \quad \frac{4}{3} = 1.333, \quad \frac{182}{144} = 1.26389, \text{ and } \frac{18029104}{14309568} = 1.2599335,$$

the true value being 1.25992105.

The mode of finding the limit of the error is given in the books on the theory of equations.

The above formulæ obviously enable us to approximate to functions of the form $\int y^n dx$. In fact our first approximation would be $\int \frac{(n-1)a^n + y}{na^{n-1}} dx$, a and y being both rational functions of x , the former chosen *pro arbitrio*. But we are by no means restricted to pure radical forms. The process applies with equal generality to the functions which are given implicitly as the roots of equations. Thus, if the given equation be $y^m + \lambda y = v$, a first approximation to $\int y dx$ is

$$\int \frac{(m-1)a^m + p}{ma^{m-1} + \lambda} dx.$$

Here a and y are supposed to be rational functions of x , and λ and p to be constant.

If we make $n=2$ in the formulæ for the simple radical, we obtain the series of arithmetic means which I gave in the memoir above quoted, viz.

$$a, \quad \frac{a^2+p}{2a}, \quad \frac{a^4+6a^2p+p^2}{2a(a^2+p)}, \quad \frac{a^8+28a^6p+70a^4p^2+28a^2p^3+p^4}{8a(a^6+7a^4p+7a^2p^2+p^3)}, \&c.$$

The harmonic series of means is obtained by multiplying the reciprocals of these into ap .

With reference to the question as to which method is the most advantageous for the purposes of elliptic functions, it may be observed that the labour attending the m th approximation in my scale is about equivalent to that of the $2m$ th in Mr. Sylvester's. It will therefore be a sufficient test to try his tenth and

my fifth term on the square root of 2. Making a or $r=1$, p or $N=2$, his tenth term gives

$$\frac{3363}{2378} = 1.41421 \ 36249;$$

my fifth term gives

$$\frac{665857}{470832} = 1.41421 \ 35623 \ 7469;$$

while the real value of $\sqrt{2}$ is 1.41421 35623 73096. My method thus appears to give twelve figures correct, while the other gives seven. I have tried several cases with a similar result.

A remarkable expression for Mr. Sylvester's successive approximations is afforded by Dr. Booth's Trigonometry of the Parabola. Divested of any peculiarities of symbol it is as follows:—let θ_m be determined by the equation

$$\int_0^{\theta_m} \frac{d\theta_m}{\cos \theta_m} = m \int_0^{\theta_1} \frac{d\theta_1}{\cos \theta_1};$$

then if we make $\sqrt{N} \cdot \sin \theta_1 = r$, the m th approximation will be expressed by $\frac{\sqrt{N}}{\sin \theta_m}$.

Kensington, Nov. 6, 1860.

LXII. *On the Decomposition of Bodies by Heat, and on Dissociation.* By M. H. SAINTE-CLAIRE DEVILLE*.

FROM what has long been known on the subject, and from facts which I myself have published, it appears well established that the action of heat finally decomposes all bodies. As this decomposition does not take place suddenly, I have assumed that the elementary or constituent molecules of a compound body, before reaching the point at which affinity ceases to act, gradually remove from each other, or dilate under the influence of heat, as do the compound or integrant molecules themselves.

1. *Simple decomposition.*—When the divergence of the elements has become so great as to overstep the sphere of activity of the force which keeps them united under the ordinary circumstances of affinity, and when, further, the molecules have not the power of combining again by the simple fact of their contact, the decomposition is definite, and shows itself by its products. We see such a case of *simple decomposition* when we subject to the action of heat ammonia, nitric acid, all nitrogen compounds in

* Communicated to the Société de Physique et d'Histoire Naturelle de Genève, September 8, 1860; translated by Dr. E. Atkinson.

general, the oxides of the precious metals, &c. ; sometimes, even, these decompositions are accompanied by a sudden disengagement of heat, as is the case with the chloride, iodide, and sulphide of nitrogen.

Decompositions which are attended with disengagement of heat are evidently an *exceptional case*, and embarrassing even to some of the most plausible theories of chemical combinations. They characterize in a *general* manner the compounds of nitrogen, which are never formed directly, or by the simple and immediate contact of their elements. The accuracy of this observation is evident for the majority of explosive nitrogenized substances, which are resolved into simple bodies at the moment of their destruction. But for non-explosive nitrogen compounds, it may be deduced from this important fact, which is confirmed by MM. Favre and Silbermann, that protoxide of nitrogen in the phenomena of combustion which it produces, develops more heat than the oxygen which it contains would furnish if burnt alone. It is clear that protoxide of nitrogen, and therefore other compounds of the same class, disengage heat at the moment of their destruction.

If two combustion experiments are made with the same quantities of potassium and sulphur, employing in succession crystallized octahedral sulphur and soft sulphur, the latter will disengage a larger quantity of heat than the octahedral sulphur, and the difference will be precisely equal to the quantity of heat observed by M. Regnault when he heated soft sulphur to 92° , and observed its temperature spontaneously rise to 110° . The quantity of heat *latent* (this is the term used, but it ought to be altered) or dissipated in the sulphur, maintains an unstable equilibrium. It may be assumed that in this respect sulphur, protoxide of nitrogen, and the fulminating compounds of nitrogen are constituted in the same manner; and it is at the moment at which this unstable equilibrium is broken by the influence of the heat itself that the calorific effect shows itself in the experiments of MM. Regnault, Favre and Silbermann, and in the destruction of the fulminating compounds of nitrogen.

On the other hand, my brother has shown how considerable is the influence of temper on the properties of sulphur. Besides soft sulphur, which was well known, and which only differs from ordinary sulphur by its *physical* properties, he discovered insoluble sulphur, which is distinguished from all the rest by a chemical property—insolubility in bisulphide of carbon. This modification has been obtained without the intervention of any other cause than the sudden change of temperature by which the phenomenon of temper is produced in steel and in explosive Dutch drops. It is therefore certain that, by tempering or sudden

cooling, bodies can be obtained whose physical properties (soft sulphur), or whose chemical properties of a certain kind (insoluble sulphur) are completely modified. These properties even bear a certain relation to the quantity of dissipated heat which the bodies retain under the influence of tempering; they also retain them in a state of unstable equilibrium, which is physically characterized by a diminution in the density, and is exhibited by the greater or less facility with which they revert to the ordinary and definite state.

I hope I make myself clear in assuming that nitrogenous compounds, and in general all those which decompose with explosion and disengagement of heat, are comparable to tempered bodies, retaining more heat than is adequate for the state of stable equilibrium of their molecules; and it is at the moment at which this stable equilibrium is destroyed that the liberated heat becomes sensible, when it is more than sufficient to produce a simple decomposition or separation of the elements. By this mode of view, the phenomena of inverse combination, or of decomposition with disengagement of heat, may be brought under the same category.

As to the formation of tempered bodies, or the fixation of this heat which renders unstable the condition of equilibrium of combined bodies, it may take place in the conditions of the nascent state, which is indispensable for the union of nitrogen with many bodies; for, from the experiments of my brother, of MM. Fordos and Gelis, of M. Berthelot, and of M. Cloez, insoluble sulphur and soft sulphur, which are obtained by tempering, are also obtained in the ordinary phenomena of decomposition during the exchange of the molecules, and under conditions which constitute the nascent state in chemical reactions.

By continuing these comparisons, we shall remove an anomaly which is truly to be regretted even in some of our most reliable chemical analogies. It might be conceived that it is nitrogen which carries with it the property which these bodies possess of becoming tempered—that it is nitrogen in the nascent state that can fix the heat borrowed from the bodies in the midst of which, and with which, it combines; so that free gaseous nitrogen and combined nitrogen are really two distinct bodies, which differ just as octahedral sulphur differs from insoluble or soft sulphur, perhaps by chemical properties, or perhaps simply by a physical property such as the density. This hypothesis would explain how it is that nitrogen possesses a density 0.972 which corresponds to two volumes of vapour, while phosphorus and arsenic only represent one volume of vapour for one equivalent, as is shown by the experiments of M. Dumas, of M. Mitscherlich, and

some of M. Troost and myself made at high temperatures. But all experiments made up to this time, and especially the latest researches of MM. Hofmann and Cahours, establish in the most incontestable manner the closest analogy between nitrogen, phosphorus, and arsenic,—but simply between these bodies in a state of combination. It might, it is true, be objected that if a certain number of difficultly volatile bodies, such as arsenic, selenium, and tellurium, only represent one volume of vapour for one equivalent, it depends on the high temperature of the boiling-point. But mercury, according to M. Dumas, cadmium, and especially zinc, which boils at 1040° , and whose density has just been determined by MM. Troost and myself at 1100° , represent two volumes of vapour for one equivalent. This objection will disappear before examples which we wish to be able to multiply.

In short, the hypothesis according to which nitrogen enters into combination in the state of a *tempered* body, like sulphur, would explain the instability of these compounds, would remove the majority of cases of inverse combination (that is, the decompositions accompanied by a disengagement of heat, which embarrass the various theories of affinity), and it would, lastly, account for the anomaly of removing nitrogen from phosphorus and arsenic, its natural allies, which is at the present day a blot in our systems of chemical analogies.

I further think that the curious phenomena observed in tempered sulphur, which my brother has minutely described, are susceptible of a wide generalization.

2. *Dissociation.*—The phenomena of decomposition under the influence of heat, of which nitrogen compounds furnish such an excellent example, are not so evident when they take place at the expense of bodies whose elements can unite during cooling, and by simple contact. Mr. Grove's experiment, which I have had occasion to repeat in conjunction with M. Debray*, by throwing into water several kilogrammes of melted platinum and lighting the explosive mixture which is abundantly disengaged, shows clearly the products of the decomposition of water at this temperature, but is far from telling what quantities of gas heat alone could produce. In fact, the melted platinum is not in contact with water, and can only decompose the small quantity of

* On one occasion this experiment gave rise to a fearful explosion; a cast-iron mortar, weighing 16 to 20 pounds and full of water, was raised to a considerable height; 500 grammes of platinum were projected to a great distance in a state of fine powder. The platinum usually traverses 30 to 40 centimetres of water, sinks in a fused or soft state to the bottom, and forms a regulus with a mammillary surface; the transparency of the water is disturbed by thousands of small gas bubbles, which can be exploded at the surface.

vapour which forms an atmosphere round the metal ; besides, as the greater part of the gas recombines by the *slow* cooling of its molecules, only that portion of gas is liberated which by a sudden cooling escapes the action of the decreasing temperature. The phenomenon is in relation with the rapidity of its development, as I have shown in the case of the dissociation of potash, soda, of chloride of magnesium, and of water itself under other circumstances.

I have shown that water comports itself like a substance whose molecules are separated, when it is submitted to a temperature of about 1000° (fusion of silver) ; on the other hand, hydrogen and oxygen *separated* produce, in combining, more heat than is necessary for melting platinum and even iridium, whose fusing-point may be taken at least at 2500° (I shall assume in what follows, but simply for facility of expression, that these 2500 degrees exactly represent the temperature of the detonating mixture itself at the moment of combustion). How is it then that the combination of hydrogen and oxygen produces sufficient heat to melt platinum, while this melted platinum can itself decompose water, and restore it to its elements, hydrogen and oxygen ? Much more, according to my experiments, does this property which water possesses of dissociating, exist at a temperature far below 2000° , a temperature which I consider to be approximately the fusing-point of platinum ; it takes place even at a temperature of the fusion of silver. How then can water be decomposed at a temperature far below that developed by the combination of hydrogen and oxygen ? This contradiction must be explained ; it appears to me to escape all the theories which have hitherto been propounded to interpret the different actions of heat on chemical combinations.

It is difficult to regard the integrant molecule of a compound body other than as composed of an assemblage of constituent molecules, separated from each other by a space, finite, though extremely small, and which further varies with the affinities of the elements concerned. Thus each compound molecule is a group of simple molecules separated from each other by finite distances, as compound molecules themselves are. Affinity is the force which keeps the constituent molecules united : it causes the *stability* of a compound ; the force of cohesion opposes the separation of the integrant molecules, and it is clear that affinity and the cohesive force would cease to act when the molecules are at a certain distance from each other.

The action of heat tends to diminish affinity as well as the force of cohesion ; by dilating bodies, it increases the distance between compound or integrant molecules until cohesion is nil ; it appears to me that it ought also to increase the di-

stance of elementary or constituent molecules by producing a dilatation, which is very small, and perhaps insensible in reference to the dilatation of the integrant molecules; but as this distance increases until it becomes equal, then superior to the radius of the sphere of activity of affinity, the compound body decomposes by heat. Between the temperature at which this decomposition takes place, and the temperature at which the body possesses all its stability, there is an interval during which the constituent molecules are at a distance comparable to the radius of the sphere of activity of the affinity. Just as the loss of cohesion suddenly reduces the entire body to a liquid state (if it is solid), so the loss of the *stability* of bodies under the influence of heat, during this interval of temperature, reduces the molecules to the particular state of superheated bodies to which I have given the name *dissociation*. The least cause can then produce decomposition; a mechanical cause, such as platinum decomposing water at 2000° ; a simple solvent action, as the solubility of oxygen in silver or in lead, produces the decomposition of water at 1000° ; lastly, a chemical action, usually elective, but devoid of this character at a high temperature, as iron assimilating oxygen in the presence of hydrogen and potassium in the decomposition of potass by Gay-Lussac and Thénard's method.

I may thus affirm that, in this interval of 1000° to 2500° , the constituent molecules of aqueous vapour are free in spite of affinity, just as from 0° to 100° (Cent.) the integrant molecules of liquid water are free in spite of the cohesive force, which is sensibly nil.

Molecular attraction produces in bodies *cohesion*, which for each special substance assumes different intensities, the values of which vary with the temperature. There are sudden changes, one of which, corresponding to the liquefaction of solid bodies, takes place at the same time that a considerable quantity of heat is absorbed, without any change in the volume of bodies, while even in some cases there is a contraction, as is the case with water, bismuth, &c. In like manner affinity communicates *stability* to compound bodies, which diminishes as the temperature increases. This variation, which is insensible within extended limits, may be considerable at a certain temperature, and fixed like the fusing-point, and without the volume of the compound becoming sensibly increased. The compound body (which I shall consider for the moment as being in a gaseous state) is then in a state of *dissociation*. It could then even absorb a considerable quantity of *latent heat of dissociation*, without its volume, and consequently the uniformity of its expansion, being notably modified.

When a liquid is boiled, there remains in the vapour no trace of the properties which characterize cohesion, and the quantity of latent heat absorbed during the passage of a gas into a gaseous state is often considerable. Similarly, when by the simple action of a high temperature a gas has been decomposed into its elements (this temperature is evidently higher than that which these elements develop during their combination), there remains in the mixture no trace of this effect of the force of affinity which constitutes the *stability* of a compound; and as the gaseous volume generally increases at the moment of the separation of the elements, there ought to be an absorption of a certain quantity of *latent heat of decomposition*.

Thus water melts at 0° , and its volume varies little; it boils at 100° , and its volume increases considerably; it dissociates at 1000° without any material deviation in its volume from Gay-Lussac's law of expansion; it decomposes above 2500° , and its volume suddenly increases in the ratio of 2 to 3.

In other words, at 0° the cohesion of water approaches its maximum; above 0° it is nil, or almost nil; at 100° it is negative. At 100° the stability of aqueous vapour is at its maximum up to 1000° , a temperature at which it almost disappears, and suddenly, in *dissociated* vapour; lastly, above 2500° it is negative in decomposed vapour: the constituent molecules then repel each other as if they were of the same kind.

If heat acts by expansion as well on the constituent as on the integrant molecules of compound bodies, we see that *stability*, like cohesion in solid bodies, may exist in gases with a positive value, which is the condition of combinations at temperatures at which we usually work; that it may exist with a volume almost nil, like cohesion in liquids (this is the state of *dissociation*, confirmed in the case of water, potass, soda, chloride of magnesium at temperatures of 1000° to 1500°); that, lastly, *stability* may become negative, like cohesion in gases, in bodies entirely decomposed by heat, like anhydrous carbonate of ammonia at 60° , ammonia at a red heat, water at 2500° .

This conception may be rendered palpable by assuming that in gaseous compounds in a stable state the constituent molecules are solid; that in dissociated bodies the molecules are liquid*; lastly, that in decomposed bodies the molecules are endowed with an indefinite mobility—in other words, the molecules themselves are gaseous.

* On this hypothesis, the constituent molecule might have a *tension* varying with the temperature, and which is greatest at the temperature of decomposition. The quantities of water decomposed in the experiments of Mr. Grove and of myself, during the state of dissociation, would be the quantities of uncondensed *vapour* corresponding to this state of tension.

This mode of expression introduces great simplicity into the scientific language relating to this question, but it does not involve a new idea. It has often guided me in connecting phenomena of total or partial decomposition, phenomena of ebullition, and of the formation of vapours with variable tensions in solids or liquids. I shall use this formula in explaining the meaning and the range of some experiments which I have made during the last winter. It is, however, to be understood that I simply rely on the analogies which I have established between affinity and the forces of cohesion; and I reserve to myself the opportunity of showing how these two forces (which are exerted on two kinds of distinct molecules—the constituent molecules, and the integrant molecules, of compound bodies) produce in analogous cases identical effects.

When two volumes of hydrogen and one volume of oxygen are combined in a medium whose temperature is 100° , two volumes of aqueous vapour are produced, whose temperature for a few moments is at least 2500° . The two effects observed are the contraction, and the elevation of temperature, of the water; the apparent physical condition (gaseous) of the three bodies, water, hydrogen, and oxygen, has not changed. Either, then, the contraction must have produced all the heat, or the hydrogen and oxygen have given out from themselves the heat which is disengaged. But the heat due to contraction would simply raise the temperature of aqueous vapour to 137° instead of to 2500° ; hence the second hypothesis can alone be admitted.

Further, chlorine and hydrogen combine without condensation; a simple ray of light is sufficient to produce an enormous elevation of temperature in the gaseous mixture. As hydrochloric acid could only borrow from its elements the heat which it has manifested, and consequently lost at the moment of combination, it is clear that it does not possess the physical state, of one at least, of the two gases which compose it; and as the distance between the molecules has not changed, it is in the constituent molecule itself that the modification must be sought from which the disengaged heat results. There are then two sources of heat in the combination of gases: 1st, the contraction in volume, the effect of which can be calculated with precision; 2ndly, an intimate modification in the constitution: with this latter I am now concerned.

In order to elucidate at least one part of the question, I have sought to ascertain what would be the influence of the heat due to contraction when the state of bodies which combine evidently undergoes no change, when, for example, two liquids combine to form a compound which is also a liquid. In order to solve this

problem, I have verified directly the temperatures produced in the liquids on which I have operated; I thus obtained numbers comparable with each other, and independent of the variation in the specific heats of bodies. The latter is only introduced into the calculation in the slight corrections which must be made in the temperatures to allow for the heat absorbed by the vessels, and for the heat expended in water evaporated in experiments made at a high temperature.

I assume that in a large number of gases (the specific heat of the product is necessary for the corrections) there is determined *the temperature of combination* of two liquid bodies, the density of the two bodies at the initial temperature, and, lastly, the coefficient of expansion of the body from the initial temperature to a point somewhat higher than the temperature of combination; we have then all the elements necessary to calculate—

1st. The volume of the components before their combination.
2ndly. The volume of the compound after combination, and in consequence of contraction.

3rdly. *The heat of contraction*, or the temperature at which the compound would have the same volume as the components.

It is clear that if there is no difference between this *temperature of contraction* thus calculated, and the *temperature of combination* observed directly, the heat disengaged by the contraction is sufficient to explain the disengagement of heat observed during combination. When this difference is positive (and this has always been the case in my numerous experiments, except with sulphate of soda which has 10 equivs. of water), it represents the loss of temperature (of *vis viva*), or the cooling effected during the act of combination, which always accompanies solution. Thus a *cooling* is sometimes observed with bodies which really combine, such as water and acetic acid.

Hence the temperature corresponding to contraction is always higher than the temperature observed directly during the act of combination—and that with almost a single exception, the cause of which is readily found: the heat disengaged in the combination of two liquids by contraction is then more than sufficient to explain the effects produced, provided, of course, that there is no change of condition.

This observation will suggest that there is, on the contrary, change of condition whenever two gases combine with disengagement of heat, producing a body also gaseous, especially when there is no contraction. In order to make my meaning clear, I shall use the mode of expression which I have previously explained.

Suppose the molecules of hydrogen and of chlorine are *liquid*, and that the constituent molecules of hydrochloric acid are *solid*,

the disengagement of heat produced during combination, and arising from this solidification, might be considerable without any change in the volume, or without any sensible variation in the distance of the compound.

Pursuing the comparisons which I have made between affinity and the force of cohesion, and supposing (which is in accordance with prevalent ideas, especially in organic chemistry*) that each molecule of hydrogen and chlorine is double, we see in fact that everything I have said about compound bodies and their dissociation applies to hydrogen and to chlorine. They are in the same physical state as water dissociated at 1000° ; and at the moment at which they combine, by rendering it sensible they lose a quantity of heat corresponding to that which the dissociated vapour of water ought to give up, in order to revert to its ordinary condition of *stability*. From this point of view I can consider hydrogen and chlorine themselves as bodies dissociated at the ordinary temperature like water heated above 1000° .

Continuing this comparison, and anticipating a research with which I am occupied relative to the temperature of decomposition of bodies which disengage a gaseous element, I think I am authorized in assimilating these phænomena to those of the ebullition of liquids, or the vaporization of non-liquifiable solids. Water deprived of air boils with great difficulty in glass, and, according to M. Donny, can even remain in the liquid state up to 140° at the ordinary pressure. But if a fragment of platinum, or some metallic powder be thrown in, the vapour is disengaged with extreme violence. It is a phænomenon of the same kind which is produced when oxide of copper or of manganese is placed in fusing chlorate of potass. The boiling-point is lowered, if I may so express myself, or, in lessening by a mechanical means the *stability* of the compound, the decomposition of the chlorate of potass may be likened to the destruction of the *cohesion* of water heated above 212° by a platinum point. This I believe to be the simplest explanation of the singular experiment which we every day perform in our laboratories in preparing oxygen from a mixture of oxide of manganese and chlorate of potass.

In a subsequent communication I shall describe the experiments which I have made with a view of defining the influence of *velocity* in chemical actions, and shall oppose the hypothesis which Berthollet has introduced into science under the name of action of *mass*.

M. Thénard, by a most ingenious and conclusive experiment,

* To make the hypothesis $\Theta=16$, $H=1$, $Cl=35.5$, is exactly the same as dividing the molecule of hydrogen and chlorine so that $O=8$, $H=\frac{1}{2}$, $Cl=17.7$, and referring these equivalents to one volume instead of to two, as is proposed at present.

has already shown the nullity of the action of mass when a very weak acid is mixed with a very stable salt,—boric acid and sulphate of potass. I have also shown that this action is nil even in the decomposition of aqueous vapour by zinc, which is as easy as that of oxide of zinc by hydrogen*. There only remain then two capital experiments in which the influence of mass intervenes; these are, on the one hand the decomposition of water by iron and the reduction of oxide of iron by hydrogen, and on the other, the inverse reactions of carbonic acid on sulphide of potassium and of sulphuretted hydrogen on carbonate of potass.

Without explaining the first phenomenon, I shall show that it is too complicated to be completely accounted for by Berthollet's hypothesis. For the other, that hypothesis is quite unnecessary. Berthollet's law of the decomposition of salts sufficiently explains it. For when a continued current of carbonic acid is passed into solution of sulphide of potassium, a layer of carbonic acid forms at the surface of the liquid, in which sulphuretted hydrogen is *volatile*, while the dissolved carbonic acid acquires *fixity*. Hence volatile sulphuretted hydrogen, fixed carbonic acid, and potass are in presence of each other. By Berthollet's law sulphuretted hydrogen would escape, and carbonate of potass be formed; but this decomposition would not take place without a continual renewal in the composition of the atmosphere being produced by a rapid current of carbonic acid; and this explains why a considerable excess of carbonic acid is needed to complete the operation, and why the bubbles of sulphuretted hydrogen are not visible in the liquid which is changed into carbonate at the ordinary temperature.

Changing the terms, the same reasoning applies to the inverse decomposition of carbonate of potass by sulphuretted hydrogen. But here, as the carbonic acid is less soluble in water than sulphuretted hydrogen, it is disengaged more rapidly in an atmosphere of sulphuretted hydrogen, and the decomposition of carbonate of potass is more rapid than that of sulphide of potassium.

LXIII. On the Molecular Changes produced by Magnetization.

By W. BEETZ †.

THE molecular changes which take place when a bar of steel or iron passes from the unmagnetic to the magnetic state, may, as M. Weber ‡ has shown, be regarded in four different

* *Annales de Chimie et de Physique*, 3rd series, vol. xliii. p. 473 and 477.

† Translated from Poggendorff's *Annalen*, September 1860.

‡ "Electrodynamische Maasbestimmungen, insbesondere über Diamagnetismus," *Abh. d. K. Sächs. Ges. d. Wissensch. phys. math. Cl.* 1852, i. p. 541.

ways, which, however, may be reduced to three essentially distinct. It may be supposed—

1. That two magnetic fluids exist, which are *capable of being set in motion independently of the ponderable body in which they reside*.

2. That two magnetic fluids exist, which can only be set in motion *in conjunction with the molecules of the ponderable bodies in which they reside*, or that there are molecular streams of a permanent character rotating with the molecules, and formed by the two electric fluids.

3. That there are two electric fluids, which may be set in unresisted motion in certain paths, *about the stationary molecules of the ponderable body*.

Of these three hypotheses, Weber felt himself compelled to regard the last as the correct one, in order to bring the fundamental phænomenon of the origin of diamagnetism into agreement with that of the origin of magnetism; he remarked, however, in the course of his work, that as far as magnetism itself is concerned, experience is in favour of the hypothesis of rotatory molecules*. According to this latter hypothesis, the amount of magnetism which can be induced in a body has a limit when the axes of all the molecular magnets of which it is composed have become parallel; whereas the existence of such a limit is incompatible with the hypothesis of the presence of an inexhaustible store of a separable neutral magnetic (or electric) fluid. Even, indeed, when the quantity of this fluid is not regarded as inexhaustible, still, according to the latter hypothesis, the ratio between the amount m of the magnetism induced, and the magnetizing power p , must remain constant until the whole of the neutral magnetism contained in the molecules of the body is separated; while, according to the other hypothesis, that ratio must be a variable quantity, continually diminishing according to some fixed law.

Lenz and Jacobi† in their researches found this ratio, $\frac{m}{p}$, constant. Joule‡ has confirmed this view. J. Müller§, on the contrary, found the ratio in question variable according to a fixed law. Buff and Zamminer|| have thrown a doubt over this conclusion; but W. Weber¶, who found it in general corroborated, expressed an opinion that the difference between these

* *Abh. d. K. Sächs. Ges. d. Wissensch.* 1852, i. p. 556.

† *Pogg. Ann.* vol. xlvii. p. 225. *Bull. de l'Ac. d. St. Pét.* vol. iv. p. 337.

‡ *Phil. Mag.* (4) vol. iii. p. 32.

§ *Pogg. Ann.* vol. lxxix. p. 337; lxxxii. p. 181. Müller, *Fortschritte der Phys.* p. 502.

|| *Lieb. u. Wöhl., Ann. der Pharm.* vol. lxxv. p. 83.

¶ *Ib.* p. 566.

contradictory results might be removed, if the experiments were carried on under precisely similar circumstances. Add to this, that Koosen* and von Feilitzsch† came, on entirely different grounds, to the same conclusion, viz. that the ratio $\frac{m}{p}$ was not constant, but approximated to a limit; and the old view of the separation of neutral magnetism present in inexhaustible quantities, must be regarded as altogether inadmissible, as must also be the case with the hypothesis of the separation of two electric fluids capable of being set in motion in quantities without limit about the molecules of the magnetized body.

There remains, therefore, the choice between two hypotheses—

1. That magnetization is due to the separation of two magnetic or electric fluids, which are only present in limited quantities.

2. That it is due to the change of position of the molecules of the body magnetized, whether these are to be regarded as molecular magnets on account of the simple separation of the two fluids, or on account of the molecular currents continually circulating about them.

As above remarked, the observations that have been made concerning the law according to which the magnitude $\frac{m}{p}$ approaches its limit, render it probable that the latter of these two hypotheses is correct: the intimate relation, which Wiedemann† found to exist between the phenomenon of magnetism and the torsion of a steel rod, are still more positively in favour of the theory of the cooperation of the material particles of the magnetized iron, since it would be hard to discover any other explanation of these relations than that suggested by the above-named physicist, the supposition, namely, of moveable particles.

The experiments I am about to communicate will furnish new and, I trust, conclusive evidence in favour of this hypothesis.

I first endeavoured to produce a magnet which should possess a maximum charge, that is to say, which should be so perfect a permanent magnet, that its magnetism could not be increased by any temporary magnetizing current.

Proceeding on the hypothesis of rotatory atoms, such a state must necessarily be attained if we so manage as not to operate on a ready-made iron bar by means of a polarizing force, but cause the separate molecules, under the influence of such a force, to arrange themselves, side by side, with their axes parallel, and thus form themselves into a magnetic bar. In order to effect

* Pogg. *Ann.* vol. lxxxv. p. 159.

† *Ib.* vol. lxxx. p. 321.

‡ *Ib.* vol. cvi. p. 161.

this, I made use of the method described by Böttger* to obtain by the electrolytic process a coherent precipitate of iron,—but with this difference, that the precipitation was conducted between the poles of a powerful magnet. A cylindrical glass vessel was divided by means of a porous partition into two cells, each of which contained a solution of iron (sulphate or chloride with sal-ammoniac). In one cell a steel plate was plunged perpendicularly as the positive pole, in the other, parallel to the first, a rectangular strip of plated metal of any kind as negative pole. The longer side of the latter was horizontal, the shorter side touched the side of the vessel. A strong magnet (sometimes a magnet of seven plates and 50 lbs. attractive power, sometimes a Logemann's magnet of 25 lbs. attractive power) was placed horizontally on the table, so that the glass tube projected partly between its limbs, and the inner polar edge of the magnet touched the outside of the glass vessel exactly where the negative plate lay against it on the inside. A simple Daniell's battery served as electromotor.

The iron plates thus obtained were hard as glass, and always magnetic. In fact, however, so strong a magnetic influence was by no means necessary to render them permanently polar; on the contrary, almost any magnetic force operating during the formation of the precipitate, was sufficient to determine its polarity. Every electrolytic iron plate, for example, is magnetic if the cathode has been placed in the direction of the magnetic inclination.

In order to test the power of the magnets thus obtained, they were placed on edge near a mirror magnetometer, so that their axis passed through the axis of rotation of the mirror, and lay magnetically east and west. Then, to measure the temporary magnetism and the changes produced in it by magnetizing currents, this whole arrangement was placed within one of the large magnetizing coils taken from a Kleiner's electromagnet. As, however, the magnetism produced by such a coil, even when weak currents were employed, turned the mirror through so great an angle that the graduated scale passed entirely out of the field of vision of the telescope, the other coil was placed near the magnetometer on the other side, and the current was made to pass through both in succession, under which circumstances the deviation of the electrometer only amounted to a few divisions of the scale. The natural position of the mirror of the magnetometer was then observed, and its positions noted when a current was made to pass through the coil, first in one direction, then in another; the bar magnet under examination was then inserted

* *Polyt. Notizbl.* vol. i. No. 4. p. 849. *Pogg. Ann.* vol. lxxvii. p. 117. *Dingl. Pol. Journ.* vol. xcix. p. 296.

in the coil, and the deviation caused by it alone was determined; and lastly, the magnetizing current was again passed through the coils, first in one direction, then in the other, and the deviations due to the temporary and permanent magnetism of the bar were read off. The numbers given below are only comparable for the same magnet, since the distance of each of the magnets was always so chosen that the reading off of the scale might be as practicable as possible.

Although in the above disposition of the experiment the precipitated particles had the opportunity of arranging themselves in regular order, I nevertheless found, contrary to my first expectation, a considerable difference between the temporary and permanent magnetism. In fact, however, assuming the hypothesis of rotatory atoms equal in magnitude and magnetic power, such atoms could only retain their parallel position, on the cessation of the magnetizing current, on one condition, namely, that all were at equal distances from each other. If this be not the case, on the cessation of the current each molecular magnet will place itself in the direction of the resultant of all the forces acting on it, as is speedily the case when the precipitated plate in the above experiment is removed from between the poles of the magnet. Moreover, even between the poles of the strongest magnets the precipitated molecules will not all arrange themselves in the same direction; for when once the cathode is covered with a thin film of precipitated iron, the atoms that follow are subjected to a double influence, viz. to that exerted at a finite distance by the poles of the magnet, and secondly, to that exerted at an insensible distance by the atoms already deposited. Now the latter force is directly opposed to the former, as any one may easily convince himself experimentally, if, instead of employing a horseshoe magnet as above described, the iron is precipitated on a magnetic steel plate. The precipitate will then be found to have a polarity contrary to that of the steel plate. If, therefore, the precipitate between the poles of a magnet be suffered to increase, each atomic layer partly cancels the polarity of the other, and it is therefore impossible to obtain a thick magnet as completely saturated as a thin one. A fully charged magnet would be produced, if a row of polarized particles could be obtained in a straight line. In order to accomplish this as nearly as possible, I stretched a thin silver wire tightly over a glass plate, and covered it with a thin layer of varnish, which, when it had become dry, I scratched away from the side turned away from the plate, so as to expose a strip of the surface of the wire as narrow as possible. The plate was then placed in the electrolytic apparatus between the poles of a magnet; and in a few minutes the wire was covered with a precipitate of iron. It

was then dried and placed within the spiral, very near the magnetometer, and its own magnetism, as well as that induced in it by a magnetizing current (produced by a 6-celled Grove's battery), was measured. In two such experiments I found—

	1.	2.
Original magnetism . . .	3·60	3·59
Temporary	3·70	3·69
Permanent	3·60	3·58

Magnetic plates broader, but equally thin with the above, consisting of precipitates of iron on the surface of silver, easily acquired a temporary magnetism greater than their original magnetism; but neither by the most powerful current, nor by rubbing them with the pole of a powerful plate-magnet, could a permanent magnetism be induced in them greater than that possessed by them originally.

	3.	4.
Length in millimetres	72	72
Breadth in millimetres	15	21
Time being precipitated . . .	10 minutes.	15 minutes.
Original magnetism	14·37	18·93
Temporary magnetism	25·18	28·70
Permanent magnetism	14·27	19·08
After friction	13·42	18·12

Thicker magnetic plates not only exhibited a greater difference between their original and temporary magnetic powers, but their permanent was greater than their original magnetism.

	5.	6.
Length	72 millims.	33 millims.
Breadth	15 "	14 "
Weight	8·825 grm.	3·246 grm.
Specific gravity	6·81	6·63
Thickness	1·2	1·06
Original magnetism	19·36	16·34
Temporary magnetism	64·5	35·38
Permanent magnetism	24·28	18·26
After friction with magnet	22·37	16·90

In experiments 3 to 6, a six-celled Grove's battery was employed.

If we suppose that magnetizing currents effect a separation of the two magnetic fluids in the metallic mass, which separation partly continues after the cessation of the current only on account of its coercive power, it is difficult to understand why different magnets give results on measurement so totally at variance. We must suppose that the coercive power of thin magnets is greater than that of thick ones, which, to say the

least, is very improbable. On the hypothesis of moveable particles, however, this difficulty entirely disappears, since the very natural supposition, that the particles of the magnet are at unequal distances from each other, completely explains the observed differences between different magnets. From the same supposition it follows, moreover, that there never can be a magnet completely saturated with *temporary* magnetism. The most powerful magnetizing currents will never be able to bring the axes of the particles into perfect parallelism with each other. The position of the axis of each particle will depend on the resultant of the force of the current and the forces exerted on it by the neighbouring molecular magnets; so that, as the strength of the current increases, the temporary magnetism approaches asymptotically to a limit. It is sufficient to refer to W. Weber's* researches on the magnitude of the ratio $\frac{m}{p}$ and the accompanying graphic delineation of his results, to be convinced of the truth of this fact.

Additional vouchers for the truth of the hypothesis of rotatory particles are furnished by the phenomena which accompany the reversal of the poles of a magnet. From a series of experiments in which a hard steel bar is magnetized by rubbing it with a magnet first in one direction and then in the other, Suchelet† came to the conclusion, that the primary magnetism is the strongest; that by reversing the poles the magnetic power is diminished, and, moreover, that it continues to diminish for every subsequent change that may be made, but only to a fixed limit. Moser‡, in communicating these experiments, adds the remark, "It is very natural that the reversal of the original polarity of a magnet should weaken its intensity, as in fact it is known to do, since, owing to the coercive power of the bar, a residuum of its original polarity is always left behind; but that the same diminution of intensity should accompany subsequent reversals of polarity does not admit of the same explanation, and is therefore, at first sight, very remarkable."

To this observation, which relates only to the permanent magnetism of a steel bar, may be added another, which is just as little explicable on the hypothesis of magnetism consisting of a separation of magnetic fluids. Wiedemann§ found that a steel bar, magnetized by means of a current of intensity I , and afterwards unmagnetized by a weaker current of intensity $-I$, cannot be further magnetized in the opposite direction by the latter current. Now the hypothesis of the separation of magnetic fluids

* *Op. cit.* p. 569 *et seq.*

† *Ann. de Chimie et de Physique*, vol. liii. p. 248.

‡ *Dove's Repertorium der Physik*, vol. ii. p. 140.

§ *Op. cit.* p. 173.

would certainly suppose that when, from any cause, the two magnetisms had neutralized each other, the further effect of the current --I would be to induce a new and contrary separation of the fluids.

A comparison between the effects of alternate magnetization on ordinary steel bars, and on magnets formed by electrolytic precipitation, led me to the following results. With respect to the temporary magnetism induced by means of magnetizing currents, electrolytic magnets behave in general precisely the same as ordinary steel bars.

Number of plate	7.	8.	9.	10.	11.	12.
Length in millimetres ...	72	72	33	31	40	25
Breadth in millimetres...	15	15	15	15		
Weight in grammes	2.764	3.38		
Specific gravity	6.81	6.73		
Calculated thickness in } millimetres	thin plate	thin plate	} 0.82	1.08	1.08	0.82
Original magnetism	+3.95	+ 8.52	+ 3.06	+10.57	+ 7.73	+ 4.95
Strength of current	51.35	50.28	-71.47	-15.32	50.04	62.31
Temporary magnetism ...	4.72	12.84	-30.13	-24.38	-33.72	26.22
	-2.30	- 8.79	-25.05	-13.94	-25.20	-20.28
	+4.30	+12.25	+29.00	+22.44	+33.02	+25.63
	-2.35	- 8.72	-25.01	-14.06	-25.12	-19.88
	+4.13	+12.15	+28.24		+32.82	+25.60
	-2.23	- 8.80	-24.94	after 25 changes	after 20 changes	-19.87
	+4.07	+11.95	+22.01		
		- 8.81	-14.43	+31.23	
		+11.70	+21.87	-24.80	
		- 8.91	-14.51		
				+21.20		

Plates 11 and 10, and plates 12 and 9, had been originally united, but had been broken apart on removing them from the deposit-plates.

For the sake of comparison, I subjoin a similar Table of the results observed on the magnetization and demagnetization of three bars of hard steel.

	1.	2.	3.
Strength of current	16.38	51.34	15.48
Temporary magnetism	+23.40	+24.11	+21.44
	-15.45	-20.49	-13.13
	+19.95	+22.41	+18.08
	-14.90	-19.40	-13.10
	+15.83	+21.80	+17.74
	-14.78	-19.10	-12.96

The succession of the numbers, in columns 9 and 11, is precisely the same as that exhibited by the steel bar. In the majority of my experiments, I found that the magnetism in the original direction continually diminished, as was the case like-

wise, though in less degree, with the magnetism in the opposite direction. Neither in the case of the electrolytic magnets, nor in the case of the steel bars, was the magnetism induced by the stream $-I$ ever equal to that induced by the stream $+I$: the difference between the two kinds of magnets is in this respect only quantitative; but in the case of electrolytic magnets, the difference between the magnetisms induced by the $-m$ and $+m$ currents is always greater than in the case of steel bars. The former may be compared with steel bars which have been first magnetized by a powerful current, and have been alternately demagnetized and remagnetized by a weaker one. In divisions 7, 8, and 10, there are small deviations from the general course, to which we shall refer hereafter.

With regard to permanent magnetism, the electrolytic magnets behave quite differently from ordinary steel-bar magnets. If a steel bar is magnetized alternately in one direction and the other, the positive as well as the negative magnetisms continually decrease, so that the two extremes lie between continually narrowing limits, which is exactly what Wiedemann found to be the case with respect to the magnetic maxima and minima of a steel bar which had been magnetized by a current I , and demagnetized by a lesser current $-I$.

If the magnetizing currents are weak, the positive values remain always much greater than the negative; the stronger the currents the more nearly the positive and negative values approximate to each other; and in the case of currents which are capable of almost completely saturating the bar, each positive value lies between two negatives. This is shown in the following Table:—

Strength of current	12·86	29·11	47·11	57·81
Permanent magnetism	+ 5·80	+ 7·11	+ 9·10	+ 14·46
	— 0·18	— 0·91	— 7·16	— 11·60
	+ 3·60	+ 5·91	+ 6·96	+ 11·43
	— 0·12	— 0·66	— 6·64	— 11·19
	+ 3·52		+ 5·90	+ 10·11
	— 0·09			— 10·09
		after 100 changes:		+ 9·03
				— 8·54
		after 100 changes:		+ 8·53
				— 8·43

On the other hand, the negative magnetism of electrolytic magnets is always less than the positive, even when they are subjected to the influence of the most powerful currents, which would have completely saturated steel bars of the same dimensions. The plates from which the following measurements have been derived were those already described, with the addition of No. 13, of which the length was 42 millims.; breadth, 15 mil-

lims.; weight, 5.17 grms.; specific gravity, 6.54; and thickness, 1.24 millim.

	7.	8.	9.	10.	11.	12.	13.
Original magnetism ...	+3.95	+8.52	+3.06	+10.57	+ 7.73	+4.95	-12.12
Strength of current ...	51.35	50.28	71.47	15.32	50.04	62.31	69.23
Permanent magnetism.	+3.95	+8.89	+5.26	+10.87	+ 9.23	+7.21	+20.11
	-1.60	-7.36	-4.10	- 2.41	- 5.97	-4.81	-14.86
	+3.34	+8.54	+4.91	+ 8.70	+ 9.04	+7.19	+16.54
	-1.62	-7.30	-3.76	- 2.32	- 5.93	-4.79	-14.29
	+3.37	+8.40	+4.70	after 25 changes	+ 9.02	+7.07	+15.89
	-1.70	-7.48	-3.72		after 20 changes	-4.63	-14.01
	+3.20	+8.30	...		+ 8.34	...	+13.79
		-7.52	...		- 5.91	...	-15.57
		+8.12	...	+ 7.73			
		-7.60	...	- 2.25			
				+ 7.65			

In none of these sets of experiments is there any such equalization of the positive and negative magnetisms as is effected by powerful currents in the case of steel bars, although, in fact, very powerful currents were applied to some of these electrolytic magnets. The currents employed in 9, 12, and 13 were excited by a six-celled Grove's battery. Divisions 7, 8, and 9 exhibit the same irregularity as occurred with respect to temporary magnetism; the values of the negative magnetism rose slightly instead of falling.

Plates 7 and 8 were very thin, and therefore very perfectly magnetized. The current which acted on 10 was very weak. If the entire magnet be again supposed to be made up of a number of molecular magnets, whose axes are at first, for the most part, parallel to each other, and in the same direction as the poles of the entire magnet, then every force which tends to reverse the polarity of the entire magnet will have to turn these axes out of their original positions. A weak current only effects this by repeated efforts (as in experiment 10); and any force succeeds in it with more difficulty in proportion as the angle at which it acts on the axes is more acute—that is to say, the more nearly the axes of the molecular magnets are parallel to the geometrical axis of the entire magnet (as in experiments 7 and 8). If, however, the current is strong and the axes of the particles deviate far from parallelism, as is the case in thick plates, then the current at once succeeds in producing a maximum effect.

Repeated changes of polarity must, however, in general increase the mobility of the particles, and therefore diminish their permanent magnetism, that is to say, their tendency to assume a particular position on the cessation of the magnetizing current, whence the general approximation of the positive and negative magnetisms to equality. It is clear that all these changes must

also exhibit themselves in values of the temporary magnetism. Steel magnets can only behave in the same way as these electrolytic plates when the force by which they were originally magnetized was greater than that subsequently employed to change their polarity. If, on the other hand, the latter force is the greater, steel bars behave, as Wiedemann has shown, as if freshly magnetized.

For the sake of comparison, I made some experiments in which the polarity of an electrolytic magnet was repeatedly reversed by rubbing with another magnet. The results were not in this case so uniform, but nevertheless they clearly proved that an electrolytic magnet, even when rubbed with the pole of a powerful permanent magnet, behaves precisely in the same way as a steel bar when rubbed with the pole of a magnet less powerful than that by which it had been originally magnetized. Even thickish plates were generally weakened by the first stroke, which magnetized them in the direction of their original magnetism; and after repeated changes they exhibited an increase in the amount of negative magnetism, precisely as in the experiments with magnetizing currents.

Plate 14. Length, 48 millims.; breadth, 30 millims.; weight, 16·42 grms.; specific gravity, 6·12; consequent thickness, 1·7 millim.

Plate 15. Length 65 millims.; breadth, 10 millims.

	14.	15.
Original magnetism	+13·50	+19·10
Attractive power of magnet.....	25 lbs.	50 lbs.
On reversing the polarity.....	+11·86	+18·84
	— 7·75	—18·15
	+11·41	+18·70
	— 9·68	—18·48
	+10·85	+18·52
	— 9·64	
	+ 9·61	

This manner of conducting the experiment admits, however, of very little precision, partly because it is impossible entirely to avoid shaking the plate, partly because it is uncertain whether the axis of the magnet which is being rubbed remains always the same.

The hypothesis of a separation of magnetic fluids consequent on magnetization is altogether insufficient to explain the foregoing results. According to that hypothesis, it is impossible to conceive that when an iron bar has been deprived of its magnetism, it should behave differently when exposed to those influences which tend to magnetize it again in the same way as at first, from

what it does when exposed to those which tend to magnetize it in the opposite direction. An electrolytic magnet, however, always retains a preference for magnetism similar to that originally impressed upon it, though this preference is not always equally strong. On the hypothesis of rotatory particles, this is precisely what might have been expected.

Erlangen, July 1860.

LXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 400.]

March 8, 1860.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communication was read :—

“On the Solar-diurnal Variation of the Magnetic Declination at Pekin.” By Major-General Edward Sabine, R.A., Treas. and V.P.R.S.

When the first year of hourly observations of the declination, January 1 to December 31st, 1841, was received at Woolwich from the Magnetic Observatory at Hobarton, and when means had been taken of the readings of the collimator-scale at the several hours in each month, and these monthly means had been collected into annual means, it was found that the mean daily motion of the declination magnet at Hobarton presented, as one of its most conspicuous and well-marked features, a double progression in the twenty-four hours, moving twice from west to east, and twice from east to west; the phases of this diurnal variation were, that the north end of the magnet moved progressively from west to east in the hours of the forenoon, and from east to west in the hours of the afternoon; and again from west to east during the early hours of the night, returning from east to west during the later hours of the night: the two easterly extremes were attained at nearly homonymous hours of the day and night, as were also the two westerly extremes; the amplitudes of the arcs traversed during the hours of the day were considerably greater than those traversed during the hours of the night.

When, in like manner, the first year of hourly observations, July 1st, 1842, to June 30th, 1843, was received from the Toronto Observatory, and the mean diurnal march of the declination magnet was examined, it was found to exhibit phenomena in striking correspondence with those at Hobarton. At Toronto also a double progression presented itself, of which the easterly extremes were attained at nearly homonymous hours, as were also the westerly; whilst the hours of extreme elongation were nearly the same (solar) hours at the two stations, but with this distinction, that the hours at which the north end of the magnet reached its extreme *easterly* elongation at Hobarton were the same, or nearly the same, as those at which it reached its extreme *westerly* elongation at Toronto, and *vice versâ*. Pursuing, therefore, the ordinary mode of designating the direction of the declination by the north end of the magnet in the southern as well as in the northern hemisphere, the diurnal motion of the magnet may be said to be in opposite directions at Hobarton and

Toronto; but if (in correspondence with our mode of speaking in regard to another magnetic element, the Inclination) the *south* end of the magnet is employed to designate the direction of the motion in the *southern* hemisphere, and the *north* end in the *northern* hemisphere, the apparent contrariety disappears, and the directions, as well as the times of the turning hours, are approximately the same at both stations.

The double progression in the diurnal variation, which was thus so distinctly and concurrently marked at stations so distant from each other, and at which the observations had been conducted with an elaborate care which would admit of no doubt as to the dependence to be placed on their general results, was at that time in great measure an unexpected and even a startling phenomenon. In the well-known description given by M. Arago (in the instructions drawn up for the voyage of the 'Bonite' in 1836) of the general phenomena of the diurnal variation in different parts of the globe, as then known, they are represented as consisting of a single progression only, with but one easterly and one westerly extreme, both occurring during the hours of the day; and no reference or allusion whatsoever is made to the existence of a double progression, or of a nocturnal interruption to the continuous motion in the one direction between the two extremes*. That the diurnal motion must be a consequence, in some way or other, of the sun's action, could not be doubted, from the fact that the period in which the variation takes place is a solar day; and whilst the progression was regarded as a single one in the twenty-four hours, it accorded sufficiently well with the prevailing notion, that the magnetic variations were produced by variations of temperature, to meet the general view, notwithstanding the grave doubts and dissents which from time to time had been expressed by those who more closely examined the phenomena of particular localities. As the existence of a well-marked double progression at some stations on the globe could, however, no longer be disputed, the difficulty which now presented itself was to explain in what way this apparently double action of the sun was produced.

On a careful examination of the diurnal motion of the declination magnet on *different days* of the years referred to at the commencement of this paper, it soon became obvious that, both at Hobarton and Toronto, many days occurred in which the diurnal march was a single progression, the nocturnal retrogression wholly disappearing; and that there were many more days in which this was more or less approximately the case. It further appeared, on subsequently comparing the observations of the *same* years at both stations, that the days most distinguished by a large and even sometimes an extravagant interruption of the otherwise continuous *single* progression, were generally the *same days at both stations*; and by extending the comparison to other though less complete series of observations

* From the omission on the part of M. Arago of any notice of a nocturnal feature, it might perhaps be inferred that the diurnal variation at *Paris* is actually, as described by him, a single progression: it seems very improbable, however, that this should be the case, since the observations at Greenwich and Kew have shown that the progression is double at those stations.

in other parts of the globe, these days were identified as those *on which magnetic storms had prevailed*; viz. days which had been distinguished by the occurrence of perturbations, often of very considerable magnitude, affecting simultaneously the magnetic elements in all parts of the globe as far as observation extended, presenting a remarkable uniformity in the effects produced at *contiguous* stations, but (as shown by the simultaneous observations at Toronto and Hobarton) manifesting a great variety both in the character and the amount of disturbance in parts of the globe distant from each other. To separate the observations affected by these exceptional and casual influences from the ordinary and what might be deemed the normal position of the declination magnet, and to study the laws of each taken *separately*, as well as in their combination, became therefore a preliminary work to the right understanding of either. That some such mode of examination would be required, had indeed been anticipated in the instructions drawn up with so much sagacity by the Committee of Physics of the Royal Society, for the guidance of those who should engage in the direction of the Colonial Magnetic Observatories then in contemplation. In the preface to those instructions, it is expressly stated that “the progressive and periodical magnetic variations are so mixed up with the *transitory* changes, that it will be impossible to separate them so as to obtain a correct knowledge and analysis of the *progressive* and *periodical*, without taking express account of and eliminating the transitory or casual.” The difficulties which impeded, and which still impede *an entire* compliance with this instruction, viz. the *perfect* elimination of the transitory and casual changes, were found to be very great. The direction which the magnet assumes when it is under the influence of a perturbation of this nature, is not distinguishable from the direction assumed under the ordinary magnetic influence, by any other criterion yet known than by the magnitude of its deflection from the mean or normal position in the same month and at the same hour; the magnitude of the abnormal deflection may be thus taken, to a certain extent, as a means of recognizing the existence of a perturbing force, and it is the only one we possess. If we employ this criterion of magnitude as manifesting a disturbed observation, and separate the observations so disturbed from the others, we must still be aware that there may exist, and that probably there do exist, amongst the body of observations from which the large disturbances have been separated, some which may be affected by the same disturbing cause or causes operating in a minor degree; and assuming the disturbances to have different laws from the general body, the unseparated minor disturbances may still impede the perfect deduction of the laws of the other class with which they are so intermixed.

But though the criterion of magnitude may not enable us to effect a *complete* separation of the two classes, it will suffice to accomplish an approximation to that end; it will separate a sufficient body of disturbed observations,—disturbed beyond the limit of any other known influential cause,—to permit the laws of the disturbing action to be investigated; and when these laws are known, we are furnished

with the means of making at least an approximate estimation of the influence exercised by the uneliminated minor disturbances on the laws which we may proceed to deduce for the class of observations from which we have not been able to effect their perfect separation.

Adopting this method of partially eliminating the influence of the magnetic storms in the observations at Hobarton and Toronto, and proceeding in the first instance with the caution suitable to a first experiment, an unnecessarily high value (as it subsequently proved) was taken as that which should distinguish a perturbed observation, and consequently but a small body of disturbed observations was separated. On a recalculation of the diurnal variation after the elimination of these, and a comparison of the results with the diurnal variation obtained previously from the whole of the observations, the character of the influence of the magnetic storms was very manifest. By the elimination of the larger disturbances, the interruption to a continuous progression from the afternoon of the one day to the morning of the following, was considerably diminished both in continuance and amount. A smaller separating value was then taken, and consequently a larger body of disturbed observations was eliminated; the effect produced was a still further reduction of the nocturnal feature. These first essays were sufficient to show that the mean effects of the magnetic storms on the declination magnet, both at Hobarton and at Toronto, attained a maximum in the early hours of the night, and constituted at both stations a very considerable part, if not the whole, of the nocturnal portion of the double progression which has been described. By still further diminishing the separating value, but still keeping it well within the limits in which no complication of disturbing causes would be hazarded, so little was found to remain of the nocturnal interruption, that I ventured, in the 1st volume of the 'Toronto Observations,' published in 1845, to express the opinion that "*if the whole influence of the magnetic storms could be eliminated from the observations, the residual portion of the diurnal variation would be a single progression with but one maximum and one minimum in the twenty-four hours.*"

The peculiar character of the magnetic storms (or disturbances as they are sometimes called), and the periodical laws exhibited in their mean effects, have been the subject of frequent investigations since 1845. It is not necessary to notice on this occasion the results of these further than as they are connected with the explanation of the phenomena of the diurnal variation, which forms the subject of this paper. It has been shown, by abundant evidence, that though apparently casual in the times of their occurrence, the magnetic storms nevertheless produce mean effects, which, when the observations of more than a very few days are combined, are seen to be of a highly-systematic character in all parts of the globe where their effects have been examined :—that the mean deflections which they occasion have always their particular hours of extreme elongation, with continuous intermediate progression :—that these hours are different in different parts of the globe, exhibiting apparently every

possible variety :—that the *disturbance diurnal variation*, as for distinction's sake it may be called, constitutes everywhere a sensible portion of the diurnal variation shown by the mean of the hourly observations from which no elimination of disturbed observations has been made :—that the diurnal variation so obtained is in fact a resultant of two diurnal variations superposed, both referable to the sun as their primary cause, but manifesting by the difference in the character of the effects produced, a distinction in the mode of operation to which they are severally due. The disturbance variation is caused by deflections which are only of occasional occurrence ; the more regular solar-diurnal variation is distinguished, on the other hand, by the regularity of its daily occurrence ; and its hours of extreme elongation, or (as they may be more familiarly termed) its turning hours are the same, or nearly the same hours of local solar time in all parts of the globe, whilst those of the disturbance variation show almost every possible variety. The relative magnitudes or proportions of the two components differ also very greatly at different stations ; and thus, by the operation of causes which as yet are but very imperfectly known, at localities where the magnetic storms are excessive, the disproportion of the components becomes excessive also, and the phases of the regular variation are rendered altogether subordinate to those of the disturbance variation. Until therefore the extension of observations shall give rise to and establish some general theory whereby the influence of the disturbances in different parts of the globe may be predicated, their particular laws at every station must be sought by a special investigation ; and no conclusion in regard to either of the components of the diurnal variation is entitled to be viewed as final which has not been preceded by such an investigation.

It has appeared desirable to enter more at length into this preliminary statement than may at first sight be thought to be required by those who have followed the different stages of the inquiries referred to, because the interpretation, which was given so far back as 1845, of the diurnal variation at Toronto and Hobarton, has scarcely received the consideration which might seem due to a laborious and apparently successful analysis of the phenomena ; and there are some eminent physicists who have framed or adopted theories for the explanation of the diurnal variation, in which theories the existence of a double progression as a universal and necessary phase is essentially implied. Amongst these, the most prominent perhaps, and the one which has obtained the widest circulation, is the theory of the R. P. A. Secchi, Director of the Observatory of the Collegio Romano, published originally in Italian in 1854 in the '*Correspondenza Scientifica*' in Rome, translated into English in the edition of 1857 of the late Dr. Nichol's '*Cyclopædia of the Physical Sciences*,' and more recently adopted in the third volume of M. de la Rive's '*Traité d'Electricité*.' In M. Secchi's memoir, the diurnal variation, with its double movement in the day and night, is ascribed to the direct action of the sun as a distant and powerful magnet, influencing the magnetic needle at different stations on the globe in a manner con-

tingent upon the direction of the magnetic meridian at each place, and producing extreme deflections to the East and to the West twice in the twenty-four hours, the turning hours being about six hours apart, and stated to be appropriately represented by a formula of two terms, one involving the sine of the hour-angle, and the other the sine of twice that angle: the phenomena of the double progression at Toronto and Hobarton are thus viewed by him as “Types of all that happens beyond the limits of the torrid zone.”

If I have represented M. Secchi's views correctly, and I think I have done so, the question between the conformity to nature of his views and mine would be tested by the facts (when they should be known) of the diurnal variation at a station in the middle latitudes where the principal influence of the magnetic storms should take place, not in the hours of the *night*, but in those of the *day*. According to my interpretation of the phenomena at Toronto and Hobarton, such a station ought to exhibit a single progression; according to M. Secchi's, a double progression with turning hours about six hours apart. Such a station would therefore furnish what might be deemed a *crucial* experiment. In the extension of our experimental knowledge which might be expected to follow from the adoption by Her Majesty's Government of the recommendations of the Royal Society and of the British Association, which have been communicated to Lord Palmerston with so much earnestness of purpose, and with so just an appreciation of their importance, by His Royal Highness the Prince Consort, as President of the British Association, it had been anticipated that it would not be long before the evidence derivable from such a station would be secured to us. I have found it, however, sooner than I had expected, or had hoped for, in the three years and ten months of hourly observations of the Declination at Pekin, from January 1, 1852, to October 31, 1855, made under the superintendence of M. Scatchkoff, attached to the Russian Embassy at Pekin, and published by our distinguished foreign member, M. Kupffer, in the volumes of the ‘*Annales de l'Observatoire Physique Central de Russie*.’ The results of these observations, as far as they bear on the questions of the general phenomena of the diurnal variation, and on the mode in which these may be explained, form the subject of the present communication.

The examination of these observations was first undertaken by me for the purpose of ascertaining, as far as possible by their means, the precise epoch of minimum in the so-called decennial period of the magnetic storms. With this view a separation was made of the larger disturbances in the usual manner, and their laws at Pekin investigated. In this process it was soon perceived that the hours of principal disturbance were those of the day, both in the easterly and in the westerly disturbance deflections; and on subsequently receiving from the computers the annual mean of the diurnal variation corresponding to the whole period of observation (in which the omission of disturbed observations during the hours of the night had been comparatively very inconsiderable), I was not surprised to find that it exhibited no trace of a double progression. The results were as follows :—

Local Astron. Time.	Variation.	Local Astron. Time.	Variation.	Local Astron. Time.	Variation.
h m		h m		h m	
0 6	1°55 W.	8 6	0°19 W.	16 6	0°53 E.
1 6	2°26 W.	9 6	0°10 W.	17 6	0°57 E.
2 6	2°32 W.	10 6	0°00	18 6	0°94 E.
3 6	1°85 W.	11 6	0°15 E.	19 6	1°51 E.
4 6	1°21 W.	12 6	0°30 E.	20 6	2°06 E.
5 6	0°65 W.	13 6	0°45 E.	21 6	2°10 E.
6 6	0°29 W.	14 6	0°49 E.	22 6	1°24 E.
7 6	0°21 W.	15 6	0°52 E.	23 6	0°20 W.

If we examine these figures, we perceive that the motion from west to east, commencing at the turning hour between 1 and 2 in the afternoon, though comparatively slow during the hours of the night, is continuous and uninterrupted until the extreme easterly elongation is reached between 8 or 9 in the following morning, and that no other turning hours intervene between those of the extreme easterly between 8 and 9 A.M. and the extreme westerly between 1 and 2 P.M.

The phases of the solar-diurnal variation, as they are shown by the Pekin Observations, may be stated as follows:—The north end of the magnet is at its extreme eastern elongation about half-past 8 in the morning; at this hour it begins to move to the west, and moves rapidly in this part of its daily course, completing its whole movement in that direction in five hours, and reaching its extreme western elongation at about half-past 1 P.M. From this hour it returns, somewhat less rapidly than in its forenoon excursion, until about 6 P.M., when the rate of progression is considerably lessened, but continues in the same direction through the hours of the night, until about 5 A.M., when it again accelerates until the eastern extreme is attained, as already stated, about 8½ A.M. There is thus a very unequal division of time in the direction of the motion, which takes five hours in the progress from east to west, and nineteen hours in returning from west to east through the same arc. We find a more equal division of time if we regard the greater or less *rapidity* of the motion: there are about twelve hours in which the motion is comparatively quick, and twelve hours in which it is comparatively slow; the quick hours being those of the day, the slow hours those of the night.

Thus far the notice we have taken of the Pekin results has been limited to the diurnal variation which we find when we take an average of the *whole* year, and which we may theoretically suppose would take place in every month of the year if the sun were always in the plane of the equator. But similar investigations had already made known to us the existence of a *semiannual inequality*, having opposite phases according as the sun has north or south declination; with turning epochs about the times of the solstices, and the phases passing into each other about the times of the equinoxes.

I have already, on a former occasion (Proceedings of the Royal Society, May 18, 1854), submitted to the consideration of the Society the concurrent evidence from three stations, Toronto, Hobarton, and St. Helena, of the existence of this inequality, and of the almost uniform character of its phases at those stations, from which I ventured to infer the probability that an inequality having a similar character would be found to be a general phenomenon. I am now able to add to the evidence which was then adduced, a representation of the semiannual inequality at three additional stations, viz. at the Cape of Good Hope, of which the particulars in detail will be found in the 2nd volume of the 'St. Helena Observations,'—at the Kew Observatory, taken from the hourly tabulations from the photographic curves obtained by the self-recording declinometer at that station,—and at Pekin, as shown in the following tabular view :—

Semiannual Means of the Solar-diurnal Variation at Pekin.

Local Astron. Time.	April to Sept.	October to March.	Local Astron. Time.	April to Sept.	October to March.	Local Astron. Time.	April to Sept.	October to March.
h m			h m			h m		
0 6	2.34 W.	0.76 W.	8 6	0.40 W.	0.02 E.	16 6	0.84 E.	0.21 E.
1 6	3.09 W.	1.44 W.	9 6	0.29 W.	0.09 E.	17 6	1.15 E.	0.00
2 6	3.06 W.	1.58 W.	10 6	0.26 W.	0.26 E.	18 6	2.07 E.	0.19 W.
3 6	2.53 W.	1.18 W.	11 6	0.08 W.	0.39 E.	19 6	3.04 E.	0.02 W.
4 6	1.79 W.	0.64 W.	12 6	0.13 E.	0.46 E.	20 6	3.46 E.	0.66 E.
5 6	1.02 W.	0.27 W.	13 6	0.43 E.	0.47 E.	21 6	2.80 E.	1.40 E.
6 6	0.43 W.	0.16 W.	14 6	0.63 E.	0.36 E.	22 6	1.15 E.	1.33 E.
7 6	0.34 W.	0.08 W.	15 6	0.73 E.	0.31 E.	23 6	0.76 W.	0.36 E.

As the correspondence of such phenomena is often far better judged of by the eye, when exhibited in the form of curves, than by the comparison of tables, I have exhibited in a diagram the phases of the semiannual inequality at the six stations, by which it will be seen that they add their confirmation to the inference which I had previously drawn. With this additional evidence of its uniform character in different parts of the globe, it may be hoped that the claim of the semiannual inequality to be received as a successful generalization from a careful and comprehensive induction may be admitted, and that as an accession to our positive knowledge it may have a recognized place amongst the facts of the diurnal variation, which have to be accounted for in the theories which may be hereafter adduced for their physical explanation.

We now, therefore, recognize three classes of phenomena derived from three different sources, which are superposed in the diurnal variation obtained from the unreduced observations, and which for a proper understanding of the whole, require to be separated from each other by a proper analysis, so that the part due to each may be distinctly ascertained : these are—1st, the mean effects of the magnetic storms ; 2nd, the semiannual inequality of the regular solar-diurnal variation ; and 3rd, the mean solar-diurnal variation of the year

into which the semiannual differences merge. The distinctive characteristics of the first, viz. the disturbance diurnal variation, have already been stated in the early part of this paper, together with the evidence they supply of being due to some modification of the solar action,—justifying their being treated as distinct and separate from the affections which constitute the more regular variation. There are also distinctive characters in the phenomena of the semiannual inequality, and in those of the mean variation, which appear to point out a difference in the mode in which the primary cause operates in producing the two classes of phenomena. For the purpose of explaining this difference, we may employ, as more likely to be generally understood, the usual custom of referring all deflections, whether in the northern or the southern hemisphere, to the north end of the magnet; we say then that, in the mean variation, the directions of the deflection are uniform throughout the year in the middle latitudes of the one hemisphere, and (although opposite) are also uniform throughout the year in the middle latitudes of the other hemisphere; whilst in the semiannual inequality, the directions of the deflection are uniform in the two hemispheres, but opposite in the two half years. In the one case the effects are hemispherical, in the other semiannual. It is this peculiarity which gives to the “April to September” branch of the semiannual inequality its analogy with the diurnal variation which prevails throughout the year in the middle latitudes of the *northern* hemisphere, and to the “October to March” branch its analogy with the diurnal variation which prevails throughout the year in the middle latitudes of the *southern* hemisphere. The analogies extend even to the small but apparently systematic difference which exists between the turning hours of the mean variation in the two hemispheres, and of the semiannual variation in the two half years. The turning hours of the variation in the northern hemisphere, and of the “April to September” semiannual branch, appear to occur systematically about an hour earlier than those of the southern hemisphere, and of the “October to March” semiannual branch. This is a connecting link which draws still nearer the analogies of which the broader features have been frequently noticed and commented upon; and is the more remarkable on account of the diversity which in other respects seems to distinguish the mode of operation by which the solar influence produces in the one case hemispherical difference with annual agreement, and in the other case semiannual difference with hemispherical agreement.

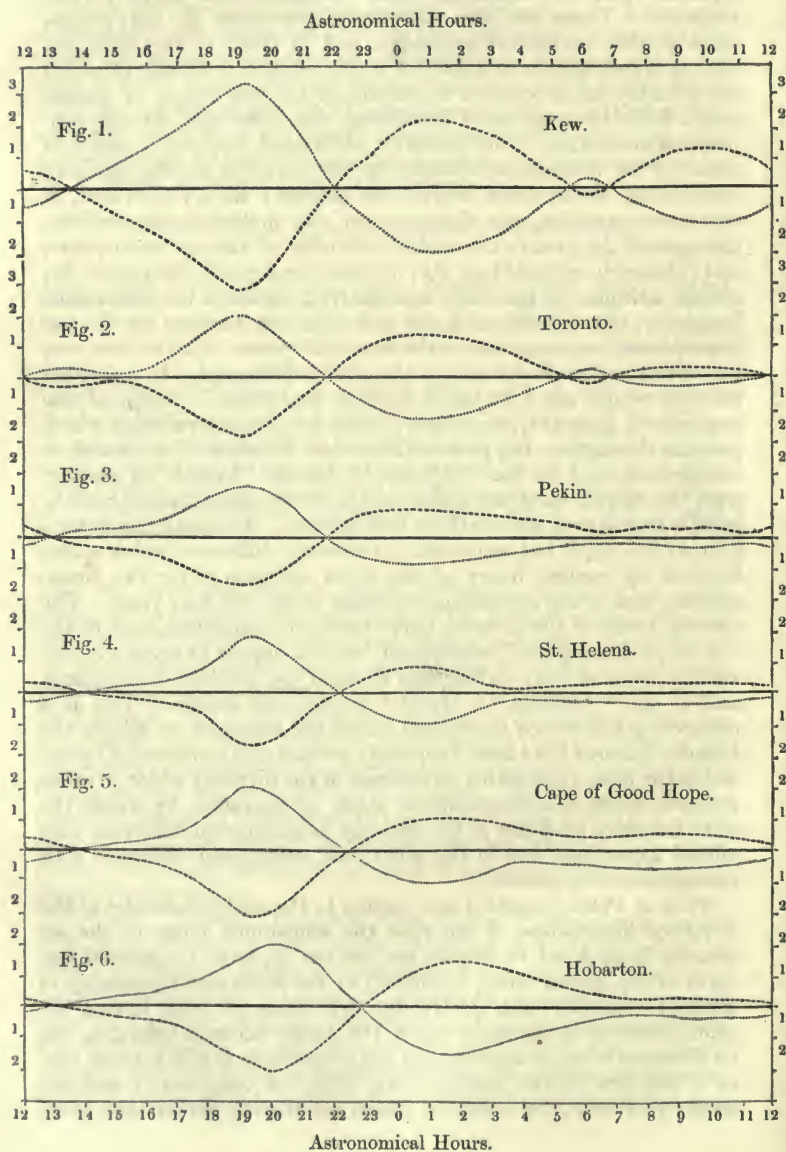
Thus at Pekin, regarded as a station in the middle latitudes of the northern hemisphere, if we view the semiannual mean of the six months from April to September, we see repeated the general features of the annual mean, reinforced by the semiannual inequality of kindred character with itself; the deflections of both having the same direction at the same hours, the range becomes enlarged, but its characteristics are unchanged; the progression is still a single one, as is the case in the annual mean, with but one easterly and one westerly extreme, the hours of which are slightly earlier than those

Solar-diurnal Variation of the Magnetic Declination.—Semiannual Inequality.

The Black line indicates the Mean Solar-diurnal Variation in the year.

The Broken line indicates Semiannual Means, October to March.

The fine dotted line indicates Semiannual Means, April to September.



of the annual mean, by reason of a particular feature of the semi-annual inequality spoken of above. When, on the other hand, we direct our attention to the semiannual mean from October to March, we see the consequence of the superposition upon the annual mean of the opposite semiannual inequality belonging to these months: this is most particularly shown in the effect produced upon the semi-annual mean by the great semiannual loop which culminates about 6 or 7 A.M. This deflection, which is opposite in direction to that appropriate to the hemisphere, prevails over it so far as to interrupt the progression, which on the mean of the year is continuous, and to produce a secondary maximum at about 7 A.M. This opposite deflection to that which is normal in the hemisphere, taking place at the hours when the semiannual inequality is greatest, is a common feature whenever, in the middle latitudes of either hemisphere, the mean diurnal variation of the one hemisphere is combined with the semiannual inequality which has the opposite analogy. The hour of principal discordance between them is always nearly the same, being determined by that of the principal deflection on the semiannual curve, which, as seen in the diagram, is nearly identical in solar time in all parts of the globe. In the semiannual mean from October to March the principal turning hours are a little later than in the annual mean, just as we have seen above that, in the April to September mean, the turning hours are a little earlier than in the annual, and for the same reason. Finally, it is this combination of the hemispherical and semiannual effects which creates the differences we observe in the amount and hours of the solar-diurnal variation in the different months in the middle latitudes of both hemispheres.

There is one more feature of some importance to the general theory of the diurnal variation, which is illustrated by the Pekin observations, and requires a brief notice. A distinction has been elsewhere pointed out (*Cosmos*, English Translation, Longman's Edition, vol. iv. p. 504, Editor's Note) between the diurnal variation of the equatorial zone and that of the middle latitudes, consisting in the circumstance that in the equatorial zone the amount of the semiannual deflection is greater than that of the hemispherical deflection at the hours when they are opposed to each other, and by its preponderance *changes the character*, instead of *simply diminishing the amount*, of the hemispherical deflection. The change in the signs of the deflection at 6 and 7 A.M. in the semiannual mean from October to March at Pekin is an illustration of this peculiarity, and ought perhaps in strictness to cause Pekin to be included in the magnetically equatorial zone; but being only just within the border, it has been found more convenient to dwell on this occasion upon the features which it has in common with stations in the middle latitudes.

The diurnal variation at Pekin reaches its extreme deflections at the same hours of solar time, as is the case at the other stations in the northern hemisphere where the phenomena have been examined with equal care. This fact is not in accord with the opinions of those physicists who regard the solar action as conditioned in its exercise by the direction of the magnetic meridian at the particular station.

In the different stations in the northern hemisphere, where the extreme deflections have been found to take place at the same hours of solar time, the differences in the direction of the magnetic meridian have not been less than 70° , equivalent to a difference of solar time of between four and five hours.

I ought not to close this paper without adverting to the success which has attended Mr. Scatchkoff's employment of native Chinese as his assistants in the work of the Pekin Observatory, holding out as it does an encouraging example to Directors of Observatories who may be similarly circumstanced. A very close test of the care and fidelity with which observations have been made and recorded is furnished by the lunar-diurnal variation, deducible from them when they have been re-arranged under the lunar hours to which they severally belong. Thus tested, the Pekin observations show no inferiority to those of other stations which have been similarly examined.

It is understood that the observations, which were discontinued at Pekin at the end of 1855, are about to be recommenced, or have been so already. It is greatly to be desired that hourly observations of the Horizontal and Vertical Forces should be combined with those of the Declination at this important station. The self-recording apparatus of the three elements which has been in action at Kew during the last two years, has been found, by the reduction of its tabulated values at hourly intervals, to be in no respect practically inferior to the method of eye-observation, whilst it possesses many advantages which are peculiarly its own. The tabulation from the Photographic Curves, as well as the reductions, might be made, if more convenient, at the central Physical Observatory at St. Petersburg.

March 15.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communication was read :—

“Analysis of my Sight, with a view to ascertain the focal power of my eyes for horizontal and for vertical rays, and to determine whether they possess a power of adjustment for different distances.”

By T. Wharton Jones, Esq., F.R.S. &c.

Besides the well-known differences of sight in respect to farness and nearness, there are differences in respect to the power of the eyes of different persons to bring the rays of light to one exact focus.

From observations and experiments in which I have for some time been engaged, I have been led to suspect that *astigmatism* or incapacity of the eye to collect all the rays of light which enter it to one exact focus, is, if not the rule of sight, at least of very common occurrence. I do not here refer to the cases in which astigmatism is of so exaggerated a character as to be a positive defect of sight.

It would be of great importance, both in a scientific and practical point of view, to possess some accurate data as to the frequency of the occurrence of astigmatism; but such can be obtained only by a number of different persons—qualified observers—contributing each an analysis of his own sight. I have thought, therefore, that by bringing under the notice of the Royal Society an analysis of my own sight, some of the Fellows and others accustomed to exact observations

might, perhaps, be induced to make similar contributions. The adjustment of the eyes for different distances being intimately connected with the question of *stigmatism* or *astigmatism*, I have included it in my analysis.

If I view a vertical and horizontal line, both equally strong and black, I see them with medium distinctness at the distance of about 10 inches.

At the distance of about $8\frac{1}{2}$ inches, I see the vertical line with greater distinctness and better definition—the greatest distinctness and best definition my eyes are capable of; but the horizontal line I see indistinctly—with much less distinctness than that with which I see any part of the figure at the distance of 10 inches. At the distance of 12 inches, I see the horizontal line with the greatest distinctness and best definition my eyes are capable of; but the vertical line I see indistinctly—with much less distinctness than that with which I see any part of the figure at the distance of 10 inches. It thus appears that my eyes collect to a focus on the retina the rays which diverge horizontally at the distance of $8\frac{1}{2}$ inches; and the rays which diverge vertically at the distance of 12 inches. Whilst seeing the vertical line with perfect distinctness and definition at the distance of $8\frac{1}{2}$ inches, I cannot alter the adjustment of the eye so as to see the horizontal line more distinctly and the vertical one less distinctly; and *vice versa*, whilst seeing the horizontal line perfectly defined at the distance of 12 inches, I cannot alter the adjustment of the eye so as to see the vertical line more distinctly and the horizontal one less.

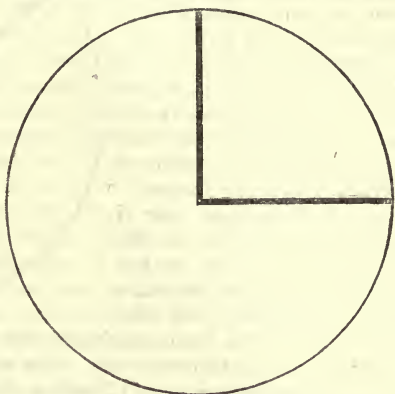
In short, I find that I have no power of altering the adjustment of my eyes. I see vertical lines with perfect distinctness and definition only at the distance of $8\frac{1}{2}$ inches, and horizontal lines with perfect distinctness and definition only at the distance of 12 inches, and both vertical and horizontal lines simultaneously with medium distinctness only at the distance of 10 inches.

At the distance of about 7 inches I see the vertical line with medium distinctness, but the horizontal line very indistinctly.

At the distance of about 14 inches I see the horizontal line with medium distinctness, but the vertical line very indistinctly.

At a nearer distance than 7 inches I see both lines indistinctly, but the vertical less so than the horizontal. At a further distance than 14 inches, on the other hand, I see both lines indistinctly, but the horizontal less so than the vertical.

If now I view two oblique lines, both of which are equally strong and black, I see both legs with medium distinctness at the distance of 10 inches.



At the distance of about $8\frac{1}{2}$ inches I see the two oblique lines equally well, but not so distinctly as at the distance of 10 inches.

At the distance of 12 inches I see the two oblique lines with much about the same distinctness as that with which I see them at the distance of $8\frac{1}{2}$ inches.

It thus appears that I cannot see either of the oblique lines with perfect distinctness and definition at any distance; but that

I can see them both simultaneously distinctly enough at any distance from $8\frac{1}{2}$ inches to 12. At a nearer distance than $8\frac{1}{2}$ inches, or a further distance than 12 inches, the distinctness diminishes, and that equally for the two lines.

I cannot by any adjustment of my eyes vary the distinctness with which I see the oblique lines at a given distance.

The preceding analysis of my sight shows that my eyes are *not monostigmatic*, that is, are not capable of collecting all the rays of light which enter them to one exact focus. It shows, on the contrary, that my eyes are *distigmatic*, that is, they have each two distinct foci to which they bring the rays, viz. one focus for horizontal rays, and one for vertical rays.

The preceding analysis also shows that my eyes do not possess any intrinsic power of adjustment whereby they can bring to foci rays diverging from a nearer or further distance than the two distances above specified for horizontal and for vertical rays.

It is true that I can see the different objects in a room distinctly enough without the aid of glasses, and that in the street or open country I can see objects distinctly enough for all practical purposes with the aid of concave glasses Nos. 2 and 3, but, critically speaking, the definition is far from being exact.

Directing my eye to an object 2 or 3 feet from me, I see it distinctly enough whilst an object in the same field of view at the distance of 10 or 12 feet is at the same moment seen very indistinctly. If now, I direct my eye to the object at the distance of 10 or 12 feet, I see it distinctly enough, but the object at the distance of 2 or 3 feet now appears very indistinct.

This is commonly considered an evidence of adjustment of the eye to the two different distances. There is, however, no real intrinsic adjustment in the case. I see distinctly enough, either the nearer or the more distant object, merely because by directing my eye to it, its image falls on the central and most sensitive part of the retina, whilst the image of the other object falls on the circumferential and least sensitive part of the retina.

It is to be observed that at neither the nearer nor the further



distance, do I see the object exactly defined on directing my eye to it. On directing my eye to the further object, I see it, of course, less defined than I do the nearer object when I direct my eye to it; but the difference is not at a glance very striking.

This experiment must not be confounded with another adduced by the late Professor Müller as a proof of the existence of an adjusting power in the eye. The experiment I refer to is as follows:—

If we regard with one eye only (the other being closed) the ends of two pins placed one before the other at different distances in the line of the axis of the eye, one will be seen distinctly when the other appears indistinct, and *vice versâ*. Both images lie in the axis of the eye, one over the other; and yet it depends on a voluntary effort, the exertion of which can be felt in the eye, whether the first or the second pin shall be seen distinctly. “The two images of the pins,” says Müller, “fall upon the same point of the retina; one lies over the other, and yet I see the nearer through the cloud-like image formed by the rays from the other more distant pin, and *vice versâ*.”

If any person is able to see the phenomena here described, he is undoubtedly endowed with an adjusting power in his eye.

I have never succeeded in seeing the phenomena myself.

In viewing objects at different distances, the sight is no doubt aided by the movements of the eyebrows, eyelids, eyeballs, and pupils; but in this we have no example of adjustment properly so called, viz. intrinsic adjustment.

That the focal power of my eye may become slowly altered, for instance by prolonged examination of near and minute objects, and again slowly return to its former state, I am satisfied; but this, again, is no example of adjustment properly so called.

P.S. It would oblige me very much, if any one into whose hands this paper may happen to fall, and who may take the trouble to analyse his sight in the manner herein described, would communicate to me the results of his observation on the following points:—

1. The distance at which the vertical line is seen with the greatest distinctness and best definition.

2. The distance at which the horizontal line is seen with the greatest distinctness and best definition.—Or,

3. If there be no difference in the distance at which the vertical and horizontal lines are seen with the greatest distinctness and best definition.—And, lastly,

4. Whether or not the observer can satisfy himself that he has the power of adjusting the eye, so as to be able to see the lines with perfect distinctness and definition at any other than one distance.

N.B. If spectacles are worn, mention the kind of glasses—whether convex or concave—and their power.

Note also if there be any difference in the sight of the two eyes.

GEOLOGICAL SOCIETY.

[Continued from p. 401.]

November 7, 1860.—L. Horner, Esq., President, in the Chair.

The following communications were read:—

1. “On the Denudation of Soft Strata.” By the Rev. O. Fisher, M.A., F.G.S.

The author first described the general features of the north-eastern portion of Essex, with its table-lands of gravel, clay valleys, and tidal rivers. The present configuration of the district cannot be due, in the author's opinion, to the action of such causes as we see now in operation on the coast, combined with a slow elevation of the land. As a rule, the sea-waves cannot excavate long narrow inlets in horizontal and homogeneous beds, such as the gravel and clay of the district under notice, but give rise to long, approximately straight lines of cliff. The rounded sides of the Essex valleys seem to show they were not formed by wave-action; nor are there any evidences of shingle-beds at the foot of the hills. Mr. Fisher believes that the surface of this district, and that of many other districts composed of yielding strata, must have been formed by a superincumbent mass of water draining off from a flat or slightly dome-shaped area. Slight depressions, cracks, or lines of readily yielding materials would determine the drainage-streams as the water retreated; and these channels would be more or less scoured out according to the velocity of the water. Where the gravel covering of such a district was cut through, the clay beneath would be channelled with a narrower valley; and where the gravel was wholly removed, the valleys would be wider and the intermediate high ground rounded instead of being flat-topped, just as is presented in those parts of the district where the clay composes the surface. Similar appearances may be seen on a small scale in the mud of a tidal river. Tidal action, however, is not, according to the author, calculated to excavate narrow valleys in horizontal beds.

Mr. Fisher suggests that the land must have been elevated by a sudden movement sufficient to have caused a rush of water from the raised portions to seek a lower level,—either the land being raised high and dry at once, or the sea-bottom raised to a higher level, though still remaining beneath water. Such an elevation might be repeated again and again, with intervals of submergence; and such conditions appear to have obtained in Norfolk as well as in Essex.

The author states that, in his opinion, escarpments, such as are so common among the secondary and tertiary beds, are rarely old cliffs, and their often rounded forms must be due to agencies similar to those which have produced the valleys of Essex. In some deep gorges of the Chalk near Dorchester the author has seen flints and great blocks of Tertiary puddingstone so arranged as to leave little doubt of their having been left by violent currents of water. The position of the Marlborough "Wethers" is also attributed by the author to torrential action.

Brick-earth is in part referred by Mr. Fisher to the deposition of sediment from turbid waters; but also in great part to the unlading of icebergs.

With regard to the manner in which the uprising of the land, which brought about these aqueous cataclysms, has been effected—whether by one slow and continued movement, or by one or more sudden movements, or by a mixed succession of these, the author argued that a slow and gradual elevation is not in accordance with the contour of the existing surface of our softer strata; that the ele-

vation of the land previous to the period of the great-mammalian fauna, when its present contour was mainly given, was not gradual ; and that, after subsequent depressions, there have been sudden elevations since that period.

Lastly, it was pointed out that sudden vertical movements of the surface on a grand scale are of as probable occurrence as those lesser movements with which we are historically acquainted, because, both in the case of strata previously unbroken and in that of strata once faulted but at rest, the pressure requisite to rupture or to fold them will accumulate enormously before they yield to it, when, after some slight slow and gradual movements, they will be thrown up or down with a sudden movement, with or without flexures, as the case may be. Thus, by mechanical considerations, the author is led to believe that the ordinary nature of movements of the earth's crust must be sudden.

2. "On an undescribed Fossil Fern from the Lower Coal-measures of Nova Scotia." By Dr. J. W. Dawson, F.G.S.

In a paper on the Lower Carboniferous rocks of British America, published in the 15th volume of the Geological Society's Journal, Dr. Dawson noticed some fragmentary plant-remains which he referred with some doubt, the one to *Schizopteris* (Brongn.), and the other to *Sphæreda* (L. and H.). With these were also fragments of a fern resembling *Sphenopteris* (*Cyclopteris*) *adiantoides* of Lindley and Hutton. Since 1858 the author has received a large series of better-preserved specimens from Mr. C. F. Hartt ; and from these he finds that what he doubtfully termed the frond of *Schizopteris* is a flattened stipe, and that the leaflets which he referred to *Sphenopteris adiantoides* really belonged to the same plant. Mr. Hartt's specimens also show that what Dr. Dawson thought to be *Sphæredæ* were attached to the subdivisions of these stipes, and are the remains of fertile pinnæ, borne on the lower part of the stipe, as in some modern ferns. This structure is something like what obtains in the Cuban *Aneimia adiantifolia*, as pointed out to the author by Prof. Eaton, of Yale College. No sporangia are seen in the fossil specimens.

Dr. Dawson offers some remarks on the difficulties of arranging this fern among the fossil *Cyclopterides*, *Næggerathia*, and *Adiantites* ; and, placing it in the genus *Cyclopteris*, he suggests that it be recognized as a subgenus (*Aneimites*) with the specific name *Acadica*.

The regularly striated and gracefully branching stipes, terminated by groups of pinnules on slender petioles, must have given to this fern a very elegant appearance. It attained a great size. One stipe is 22 inches in diameter, where it expands to unite with the stem ; and it attains a length of 21 inches before it branches. The frond must have been at least 3 feet broad. The specimens are extremely numerous at Horton.

The author then notices that the long slender leaves so common in the Coal-measures of Nova Scotia, and hithertocalled *Poacites*, though sometimes like the stipes of *Aneimites*, are probably leaves of *Cordaites*.

On some specimens of *Aneimites Acadica*, markings like those made by insects have been observed ; also a specimen of the *Spirorbis carbonarius*.

3. "On the Sections of Strata exposed in the Excavations for the South High-level Sewer at Dulwich; with Notices of the Fossils found there and at Peckham." By Charles Rickman, Esq.

In the autumn of 1859, open cuttings were made at Peckham, in connexion with the "Effra branch of the Great South High-level Sewer," for the "main drainage" of the metropolis south of the Thames; and in the following spring a tunnel (330 yards in length) was being constructed under the Five-fields at Dulwich. The beds exposed in both sections belonged to the "Woolwich and Reading Series" of the Lower London Tertiaries (Prestwich).

Four shafts were sunk to facilitate the driving of the tunnel; and the following beds were exposed; but, as some of the beds are not persistent, but die out even within the extent of the tunnel, the several shafts differed as to the sections obtained from them.

1. Soil, 9 inches. 2. Loamy clay (probably London Clay); 12 ft. Not in shaft No. 1 (the most easterly), nor in No. 4 (the most westerly), owing to the convex surface of the ground. 3. Light-coloured clay; 6 to 9 ft. 4. Reddish sand; 5 ft. Not in No. 4 shaft. 5. Dark clay; 1 ft. 10 in. 6. Blue clay; 2 ft. Not in No. 4. 7. Dark clay; 1 ft. In No. 1 only. 8. Paludina-bed; 6 to 15 inches. Fossils: *Pitharella Rickmani* (Edwards), *Paludina lenta*, *P. aspera* (?). Bones and scales of Fish. Leaves. 9. Cyrena-bed; 1 to 2 ft. *Cyrena cuneiformis*, &c. 10. Oyster-bed; 1 to 3 ft. *Ostrea tenera*, *O. pulchra*, *O. Bellovacina*, *O. elephantopus*, *O. edulina*, *Byssarca Cailliaudi* (?), *Cyrena cuneiformis*, *C. deperdita*, *C. cordata*, *C. obovata*, *Melania inquinata*, *Melanopsis brevis*, *Modiola elegans*, *Fusus* (?), *Calyptrea trochiformis*, *Corbula*. 11. Loamy sand; 8 in. In No. 4 only. 12. Red sand; 2 ft. In No. 4 only. 13. Blue clay; 2 ft. 6 in. Leaves. 14. Dark sand; 8 to 28 in. 15. Blue clay; 18 in. to 9 ft. Laminated; rich in Leaves, Lignite, Seed-vessels. *Rissoa*, *Cyrena Dulwichensis* (Rickman). 16. Dark sand; 2 to 4 ft. 17. Light-coloured clay; 2 ft. 6 in. In No. 4 only. 18. Shell-rock; 4 ft. thick, sometimes intercalated with stiff blue clay. *Cyrena Dulwichensis* (Rickman), *C. cordata*, *C. deperdita*, *C. cuneiformis*, *Melania inquinata*, *Melanopsis*, *Neritina*, *Pitharella Rickmani* (Edwards), *Unio*, *Teredines* in Lignite, Scutes of Crocodile, Fish-scales, Cheilonian and Mammalian bones. 19. Clay; 14 ft. and more. Reached only by the main shaft, No. 3, which appears to have been sunk at the apex of a low anticlinal; the beds gently dipping away E. and W.

All the fossils appear in their respective beds both at Peckham and Dulwich.

LXV. Intelligence and Miscellaneous Articles.

ON THE RELATION BETWEEN THE DIRECTION OF THE VIBRATION OF LIGHT AND THE PLANE OF POLARIZATION. BY F. EISENLOHR.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—I observe that Mr. Stokes has some remarks in your Periodical for December 1859, upon a paper of mine, which was first published in the 104th volume of Poggendorff's *Annalen*, and trans-

lated in the September Number of your Magazine for 1859. As some passages of this translation do not agree with the original, in order to express my own views correctly I beg your permission to insert the following corrections and explanations:—

- P. 187 of the translation, line 9, *instead of* it was *read* it would be.
- 188, line 18 from below, *instead of* to another occasion *read* to the immediately following paper (in Poggendorff's *Annalen*).
- 188, line 4 from below, *instead of* if this be so *read* if we do this (that is, if we consider the stratum infinitely close to the surface).

If Mr. Stokes will compare these corrections, he will find that I never assumed "that the diffracted ray may be regarded as produced by an incident ray agreeing in direction of propagation with an incident ray which would produce the diffracted ray by regular refraction, but in direction of vibration with the actual incident ray." On the contrary, I said that such a ray would represent the incident ray in its effects on the diffracted ray only in the surface of the glass, not in the stratum infinitely close to that surface. The calculus by which I have obtained the formula of p. 189, I have given in the paper which in Poggendorff's *Annalen* succeeds the article translated in this Periodical; it rests only on the hypothesis that, at the bounding surface of the two mediums, the motion, and the differential of the motion with respect to the normal on the surface, must be the same for the two mediums in the intervals of the grating, but absolutely zero at the bars of the grating. Now we may regard the motion in the second medium as composed of an infinite multitude of motions propagated in all possible directions, and we may determine, according to the theory of Fourier's double integrals, the amplitude of each of these undular motions by the above-named conditions for the surface. In my paper I have given for the sake of simplicity a somewhat abridged calculus, which, as it rests on the same principles, leads necessarily to the same results. Since then, by a quotation in the paper of Mr. Stokes "On the Dynamical Theory of Diffraction" (Cambridge Transactions, vol. ix.), which the author had the kindness to present me with, I became acquainted with a paper by M. Cauchy (*Comptes Rendus de l'Académie des Sciences*, vol. xv. p. 670), in which the author had already explained the principles of the above-given solution, but without giving any of the analyses*. Nevertheless the result of M. Cauchy's theory (that is, the formula of my paper) is at variance with that of Mr. Stokes's investigation; only I would not concede that for that reason alone the assumption on which it rests cannot be admitted, though I value very highly the merit of Mr. Stokes's disquisition.

The theory of Mr. Stokes, as does that of M. Cauchy, rests on the assumption that the screen which contains the diffracting apertures is infinitely thin; so that the motion at the issue of the apertures, where it occasions secondary waves in the second medium, is the same as at the entrance, or as in the unbroken incident wave. It appears very

* It was also on this occasion I saw that, when I said that Mr. Stokes had not communicated the details of his experiments, I was in error, into which I was led by an extract of Mr. Stokes's paper, and for which I beg to be excused.

improbable that any sensible error will arise from this assumption, especially if the thickness of the screen (soot or any other opaque medium) may be neglected in comparison with the breadth of the apertures, though a further theoretical examination of the question would be desirable. But both theories differ in what regards the propagation of the secondary waves after their having left the apertures. Mr. Stokes assumes that this propagation is the same as if the screen were not there; indeed he says (in the introduction of the Dynamical Theory of Diffraction), "It is an hypothesis (exceedingly probable *à priori*) that we may find the disturbance in front of the aperture by merely taking the aggregate of the disturbances due to all the secondary waves, each secondary wave proceeding as if the screen were away, in other words, that the effect of the screen is merely to stop a certain portion of the incident light;" and further (§ 31 at the end), "In determining the law of disturbance in a secondary wave we have nothing to do with the aperture." On the contrary, according to the theory of M. Cauchy and that given in my paper, the secondary waves produced by the motion at the issue of the apertures must in proceeding satisfy the condition that the motion resulting from all secondary waves is, on that side of the screen or of the bars of the grating which regards the second medium, absolutely zero. It is true that this condition is fulfilled only when the screen is of a substance which absorbs the light in a very high degree (as, for example, soot or silver); but for the majority of the substances used as screens the error will not be sensible.

To this difference in the principles of both theories I should trace the difference of the results; but after examining that difference, I must give it as my opinion that the assumption of M. Cauchy, while it is confirmed by experiment, is also the true interpretation of the conditions under which the experiment is made; whereas that of Mr. Stokes, that the secondary waves produced at the apertures may be regarded as proceeding freely in all directions, seems to me to be contrary to these conditions, and the consequences derived from it to be therefore erroneous.

Yet with the opinion expressed by Mr. Stokes, "that the whole question must be subjected to a thoroughly searching experimental investigation before physical conclusions can safely be drawn from the phenomena," I cannot but concur.

I am, Gentlemen,

Yours respectfully,

Heidelberg, August 14, 1860.

F. EISENLOHR.

ON THE SPECIFIC AND LATENT HEAT OF NAPHTHALINE.

BY M. ALLUARD.

Repeated investigations have led Alluard to the following results. Naphthaline melts at $79^{\circ}9$, and solidifies at the same temperature. Its specific heat in the solid form is 0.3249 between 20° and 66° , and 0.3207 between 0° and 20° ; for the liquid condition between 80° and 130° it is 0.4176. Its latent heat of fusion is 35.68 thermal units. Its specific gravity in the liquid state at $99^{\circ}02$ is 0.9628. —Liebig's *Annalen*, February 1860.

THE
LONDON, EDINBURGH AND DUBLIN
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AND
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SUPPLEMENT TO VOL. XX. FOURTH SERIES.

LXVI. *On the Pressure of Earth on Retention Walls.* By
J. J. SYLVESTER, *Professor of Mathematics at the Royal
Military Academy*.*

PART I. *Critique of the Hypothesis of Parallel "Planes of
Rupture."*

THE ensuing investigation deals with the pressure of *Mathematical* earth. By mathematical earth, I mean earth treated according to the idea of Coulomb, viz. as a *continuous*† mass separable by planes in all directions, but whose separating surfaces exert upon one another forces consisting of two parts, one of the nature of ordinary friction, the other of so-called cohesion. Of the latter, for greater simplicity, I shall commence with taking no account, so that the matter with which we have to deal becomes, so to say, "a frictional fluid." If we isolate in idea any element of this fluid—suppose, to fix the ideas, a molecule bounded by plane faces, this molecule will be kept at rest by its own weight, the pressures on the several faces, and the forces of friction acting along these faces: these last-named forces are limited not to exceed the product of the corresponding pressures by a certain coefficient, termed the coefficient of friction.

In order to render the inquiry before us quite definite, let us begin with supposing two vertical side walls and a back of solid immoveable masonry, between which the earth is piled up in a determinate form, fronted by a pier of given specific gravity, whose *minimum* thickness is to be determined by the condition that it may just suffice to prevent the pier from being either forced forward or turned round over its further edge. The earth is thus of course supposed to have only one free face, being entirely supported at the sides and the back by the masonry just spoken of. The problem then that we have to solve is

* Communicated by the Author.

† The only *essential* quality of our mathematical earth which differentiates it from actual vulgar earth is this of *continuity*.

evidently the following:—"Of all the possible states of equilibrium of the earth consistent with the assigned conditions, to determine that one which shall make the greater of two quantities to be named the least possible,"—one of these quantities being the thickness of the wall determined by the condition that its friction with the ground shall be just equal to the sum of the horizontal pressures on the wall, the other by the condition that its moment about the edge most remote from the earth shall be just equal to the sum of the moments of the entire thrust upon the wall at each several element thereof in respect to the same edge.

Whenever Coulomb's method leads to a right solution of the problem of revetments, the thrusts on the several elements of the wall will be all parallel; and it may easily be seen that, in solving the problem for this case, we are solving the problem of making the statical sum of the thrusts a minimum; and the result will be the same, whether the pier can only be pushed bodily on its base, or can only turn over an edge, or can do both one and the other. But it must obviously be erroneous to assume as a universal principle, that in the state bordering upon motion, or what is going still further, in a state antecedent to this, the statical sum of the pressures will be a minimum; and if Mr. Moseley's "principle of least resistance," quoted by Professor Rankine, means this, I have no scruple in proclaiming my entire dissent from such an assumption. I do not here enter at all into the question of determining pressure, except in the state of equilibrium bordering upon motion; and in that state common sense points out that it is not the pressure or sum of pressures, but the effect of such pressure or pressures in inducing motion in a certain possible manner, or in any one out of a choice of possible manners, that governs the determination of the minimum. This principle of least resistance is one of the shoals upon which Mr. Rankine's investigation appears to me to have split.

Be it observed that the only physical assumption which I propose is this, that if *equilibrium can be preserved consistently with the imposed conditions, equilibrium will be preserved*. Without such a supposition the question would be incapable of treatment without further laws regulating the interior forces than we suppose given. The legitimacy of such an assumption cannot, I think, be seriously called into question, and once made, the problem of determining the wall's thickness becomes a purely mathematical question; one undoubtedly of great difficulty, but perfectly determinate, and falling under the dominion of the Calculus of Variations, as will easily be recognized from the circumstance that the integration of the general equations of equilibrium, if it could be performed, would necessarily contain

arbitrary functions, whose form would have to be assigned so as to make a certain quantity or the greatest of a set of quantities a minimum; but the peculiar manner in which the internal forces are defined as subject to satisfy not an equation or system of equations, but a law of inequality, must render it a task exceeding the present powers, at all events, of the writer of this paper, to arrive at a result by the direct application of the Calculus referred to. In order to pave the way to the discussion of the more general inquiry, I shall commence with examining whether under any and what circumstances the forced solution of Coulomb and his followers, founded upon the notion of what have been (it seems to me incautiously) termed planes of fracture or rupture (but which really mean no more than planes for which friction at each point thereof is acting with its utmost energy, *i. e.* if we please so to say, planes of greatest frictional energy*), is the true

* It is obvious that the notion of the planes in question being the planes in which the earth would begin with crumbling, if the equilibrium were disturbed by the wall giving way (for such is the idea intended to be conveyed by their being called planes of rupture), is quite irrelevant to the determination of their position, and to the solution of the question of the thrust in the wall. But such a notion in itself is objectionable, as assuming a physical fact for which there is no just ground. The idea, or rather I may say the metaphysical process, which unconsciously has swayed Coulomb and his followers to give them this name, appears to me to be the following. "Since it is only along these planes that friction is acting at its full energy, and since, when motion ensues, friction must be acting at its full energy, therefore a change must have taken place in the friction of any other plane before motion can take place along it, which change does not take place along the planes in question. Now every change must operate in time, therefore the motion must have begun along the planes of greatest friction before it can have taken place along any other." But it is a most dangerous proceeding, and fraught with errors familiar to mathematicians, to attempt to reason from the conditions of equilibrium to those of incipient motion; and that dynamical considerations, and not statical, must decide the incipient directions of the motion in the case before us, will be obvious when we reflect that the friction might be supposed to become *nil*, and then we should be treating of a perfect fluid, in which case the planes of rupture disappear, but none the less would motion take place in determinate directions on any wall of the reservoir containing the fluid giving way. A notable example of the important distinction between rest and equilibrium is afforded by the question (which, I am informed, originated in Caius College, Cambridge) of finding the tension of a rope by which a bucket full of water, with a cork tied to its bottom, is fastened to a fixed point, at the moment when the fastening is cut or gives way. At that moment the vertical pressure in the bottom of the bucket, supposing the specific gravity of the cork to be one-fourth that of water, if it could be estimated on statical principles, *i. e.* with reference to the elevation of the surface of the fluid [and some non-mathematical physicists might easily suppose it could be so estimated, since motion has not *yet taken place*, but is only *imminent*], would be the weight of the bucket together with that of the water, together with four times that of the cork, and so it would appear as if the tension would be increased by the cutting of the

solution ; that is to say, I shall investigate under what conditions the surfaces of "rupture" or "of greatest energy of friction" are or can be planes ; and I shall easily be able to ascertain these conditions, and to prove that when they are satisfied (but not otherwise) the results of the received theory are exact.

Professor Rankine, in the light in which he appears in a paper published in the Transactions of the Royal Society, is not to be ranked among those whom I have called the followers of Coulomb. He is entitled to the merit of having perceived that the received hypothesis rested on no solid foundation, and of having been the first (publicly at least) to assert that the equations of internal equilibrium must be resorted to for the satisfactory discussion of the question ; but, notwithstanding the sincere esteem in which I hold the great abilities of this gentleman, I have been compelled to come to the conclusion, and trust to be able to satisfy himself, that the use he has made of these equations is illusory, and that his results bear upon their very face a demonstrable character of error.

Under the supposed *data*, it is, if not obvious, at all events assumed by all writers on the subject, that the equilibrium of every vertical section of the earth, parallel to the side walls, may be determined *per se*, and that we may treat the question as one regarding space of only two dimensions. I shall therefore, with a view to clearness, treat of the equilibrium of any one such section ; the molecules, whose equilibrium is to be considered, will be spoken of as bounded by lines instead of planes, and so we shall speak of lines instead of planes of "rupture," and we may thus conform our language to the relations of the figure actually represented upon the paper.

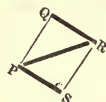
For the benefit of those to whom the conditions of molecular equilibrium are new, it may be well to indicate briefly how they may be obtained, still keeping within our prescribed framework of two-dimensioned space (although the reader will not experience the slightest difficulty in extending them to space of three dimensions)*. Through any point in the interior of the

string, whereas, in fact, precisely the contrary effect will take place ; for since downward momentum must result from the impending motion of the cork upwards and the water downwards, part of the weight of the water and cork is spent as downward moving force, and consequently only a portion remains to act as vertical pressure upon the bucket, just as an air-cushion will press with less force than its weight on the seat which bears it, when, in consequence of the air being let out, part of the weight is being expended in lowering the top of the cushion.

* I have purposely begun with the beginning, because I wish to give perfect precision to the terms Thrust, Pressure, and Stress, as I shall use them. Some recent authors on mechanics have wished to distinguish force measured

plane-mass at rest, imagine a small rectilinear element to be drawn. The entire molecular force exerted on this actual element might be termed the thrust; but by the thrust I shall understand *the unit of thrust*, corresponding to the well-known conception of *unit of pressure* for the particular case of a fluid mass. This thrust (or unit of thrust) may be imagined separated into two parts, one perpendicular to the element, which may be termed the *pressure*, the other parallel to it, which may be termed *face-force* [for the case we shall have more especially to consider, the face-force receives the name of *friction*, and is limited to be less than the *pressure* multiplied by the so-called coefficient of friction]. As the element acted upon turns round, the thrust changes in magnitude and direction, and to the totality of the thrusts going forth in all directions from a given point we may give the name of *stress**. We shall now be able to obtain two sorts of conditions—one giving the necessary law connecting the various *thrusts* of the same *stress*, the other expressing the law of the variation of the pressure and facial force (together constituting the thrust) upon an element given in direction in passing from one stress to another; we may call these respectively the equations of distribution and the equations of variation.

Let P Q R S be any infinitely small molecule bounded by lines at right angles to one another. Since this is kept at rest by its own weight, by the lines of pressures perpendicular to Q R and P S, and the other pair perpendicular to P Q and R S, and by the facial forces acting along P Q, Q R, R S, S P respectively, if we call f the face-force [*i.e.* the unit of face-force] on P S, it is obvious that the corresponding quantity for Q R will differ



statically from force measured by acceleration, by giving to the former the name of pressure. But surely unnecessary confusion is introduced into mechanical language when we are thereby reduced to speak of the pressure of friction, and ought to enunciate the cardinal law of friction by stating that the *pressure of friction* bears to the *pressure of pressure* a certain limiting relation. I acknowledge an objection scarcely less valid (except that it has antiquity to plead in excuse) to the use of the term accelerating force; as we may be thereby reduced to speak of the accelerating force of a retarding influence, as friction, or of an influence which does not necessarily either accelerate or retard, as in the case of a centripetal pull upon a body moving uniformly in a circle. I think this difficulty in language may be met to some extent by giving to force, usually called accelerative, the designation of *alterative*, and to force measured by weight or momentum that of *quantitative* force. There is no magic in names, however well selected, but there may be a great deal of mischief arising out of a confused and uncertain nomenclature.

* Thus, stress stands in somewhat the same relation to its component thrusts, as a radiant point to the luminous rays which it emits.

from it by an infinitely small quantity; in like manner f' may be taken as the face-force on $Q R$, $P S$ respectively. Hence the couples whose moments are $(f \cdot Q R) \times Q P$ and $(f' \cdot P Q) Q R$ respectively must be equal and opposite, or in other words, f being understood to act to or from Q , according as f' acts to or from Q , we must have $f=f'$. To fix the ideas, conceive the face-forces to tend towards Q . Let us now consider the equilibrium of the triangular molecule $P Q R$. Call the pressure on $P Q$ (R), the pressure on $R Q$ (P) the face-force on $P Q$ or $Q R$ (Q). In comparison with the thrusts on the faces of our triangular molecule, gravity or other impressed forces may be neglected as giving rise to quantities of an inferior order of smallness.

Let $Q P$, $Q R$ be regarded as two fixed rectangular axes, and let $Q P R = \theta$. Let the pressure and face-force on $P R$ (always understanding thereby the units of such forces) be called N and F respectively (F , to fix the ideas, being taken to act from P to R). Then resolving the forces perpendicular to $P R$, we obtain

$$N \cdot P R = R \cdot P Q \cdot \cos Q P R + P \cdot Q R \cos Q R P \\ + Q \cdot P Q \sin Q P R + Q \cdot Q R \sin Q R P,$$

or

$$N = R (\cos \theta)^2 + 2 Q \cos \theta \sin \theta + P (\sin \theta)^2;$$

and resolving parallel to $P R$, we have

$$F \times P R = R \cdot P Q \sin Q P R - P \cdot Q R \sin Q R P \\ + Q \cdot P Q \cos Q P R - Q \cdot Q R \cos Q R P,$$

or

$$F = (R - P) \sin \theta \cos \theta.$$

Imagine now $Q R R$ to be represented by a single point O . R , P are respectively the pressures, and N the face-force ("units of pressures and of face-force") on elements drawn in the orthogonal directions $O X$, $O Y$; N the pressure, and F the face-force on an element drawn in the direction $O P$, making an angle θ with $O X$. Obviously, therefore, if we draw in all directions from O lines whose lengths are as the inverse square roots of the pressure-part of the thrust acting on those lines, calling the length of line corresponding to θ , r , we have

$$\frac{1}{r^2} = R (\cos \theta)^2 + 2 Q \sin \theta \cos \theta + P (\sin \theta)^2,$$

R , Q , P being constant quantities.

Consequently the locus of the extremities of these lines is a conic; and taking new axes of coordinates in the directions of the principal axes of this conic, and understanding by R and P the pressures perpendicular to those axes respectively, the equations obtained assume the form

$$N = R (\cos \theta)^2 + P (\sin \theta)^2, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$F = (R - P) \sin \theta \cdot \cos \theta; \quad . \quad . \quad . \quad . \quad . \quad (2)$$

showing that in elements in the directions of the principal axes the face-forces vanish, and the thrusts become purely pressures, *i. e.* forces perpendicular to the surfaces upon which they act. R and P are of course essentially positive, as otherwise the molecules would be subject to a force of separation instead of compression, and consequently the conic in question is an ellipse.

The total value of the thrust $= \sqrt{N^2 + F^2}$

$$= \sqrt{R^2 (\cos \theta)^2 + P^2 (\sin \theta)^2}. \quad . \quad . \quad . \quad . \quad . \quad (\alpha)$$

R and P will evidently be in the directions in which, for a given point, the entire thrust, as well as the pressure-part of it, are the least and greatest. These directions may be said to be those of "principal thrust." If we start from any point and proceed from that point always in the direction of a line of principal thrust so as to form a continuous curve, two such curves cutting each other at right angles will intersect every point of the mass at rest, of which, in the case of mathematical earth, I may state, by way of anticipation, that only one can cut the free surface when that surface is supposed to form part of a horizontal plane.

These lines may also be termed the principal lines of pressure, or simply the lines of pressure; and this name may be considered indifferently to have reference either to the fact that the thrust *in the direction of the tangent* at any point in any such curve is the thrust acting upon the normal, or to the fact that the thrust *upon the tangent* at any point is in the direction of the normal; as either one of such conditions implies the other.

The cosine of the angle between the pressure and the thrust will be

$$\frac{R (\cos \theta)^2 + P (\sin \theta)^2}{\sqrt{R^2 (\cos \theta)^2 + P^2 (\sin \theta)^2}};$$

which, calling the principal semiaxes of the ellipse referred to a and b respectively, and the rectangular coordinates of any point therein x and y , becomes

$$\frac{\frac{x^2}{a^2} + \frac{y^2}{b^2}}{\sqrt{\frac{x^2}{a^4} + \frac{y^2}{b^4}}} = \frac{1}{\sqrt{\frac{x^2}{a^4} + \frac{y^2}{b^4}}}$$

which is equal to the perpendicular from the centre on the tangent divided by the radius vector, showing that the direction of the thrust on any radius of the ellipse in question is in the direction of the conjugate diameter, whereby it is seen that the

line of thrust and the line thrust upon stand in a reciprocal relation to each other.

I may add the cursory remark as regards the value of the total thrusts in the case more immediately before us, that [as is apparent from the equation (α)] they will be represented in relative magnitude by the radius vector drawn in the direction of the line thrust upon, to meet, not the ellipse of pressures just described, but another ellipse whose major and minor axes are to one another in the duplicate ratio of the other two.

If we wish, however, to present the above results in a form more immediately translatable into the actual case of nature, I mean that of space with three dimensions, it becomes expedient to use a different ellipse, or rather the same ellipse in another position, to represent the stress at any point.

In the equations above found, connecting N and F with P, Q, R, θ is the angle made with a fixed axis, not by the line of pressure R , but by the element on which this pressure is exerted. Let ϕ be the angle made by the pressure itself, so that $\phi = \theta + \frac{\pi}{2}$, then we have

$$N = P (\cos \phi)^2 - 2Q \sin \phi \cos \phi + R (\sin \phi)^2$$

$$F = (P - R) \sin \phi \cos \phi.$$

And the same process as has been already employed will serve to show that we may construct an ellipse such that the inverse square of the radius vector in every direction may represent the magnitude of the *pressure* in that direction (*i.e.* the magnitude of the normal part of the thrust upon the element perpendicular to that direction), and in this ellipse the radius vector and perpendicular to the tangent at each point will represent the corresponding directions of pressure and thrust, which obviously will coincide for the directions of greatest and least pressure.

If, now, we go out into space of three dimensions, it will readily be anticipated, and may easily be proved, that an ellipsoid whose radii vectores represent the relative magnitudes of the inverse square roots of the pressures takes the place of the ellipse, the thrusts and pressures correspond respectively (in direction) to the normal and radius vector at each point, and in three directions, at right angles to each other, these latter come together.

It is desirable that the reader should bear in mind that the ellipse of which I have spoken is in fact only a principal section of this ellipsoid. The assumption which (following in the track of my predecessors) I shall make, that the greatest energy of

friction exerted at any point will be exerted in some direction in a vertical plane parallel to the revetment wall, will be seen from what follows a little further on, to imply that every such plane contains the radius vector which makes the greatest angle with the normal, and consequently the section of the ellipsoid of stress with which we are dealing will be the plane of greatest and least thrust, or greatest and least pressure. By way of aid to the imagination in seizing this subtle conception of stress (a real conquest in physical ideology due to the last quarter of the present century, although its first germ may be recognized in the much earlier molecular view of the circumambient pressures round about each internal point of a perfect fluid), I have gone thus briefly into the generation of the ellipse and ellipsoid above described; but I shall have very little occasion, except for occasional facility of reference, to have resort to them, as the equations (1) and (2) will suffice for my purpose in the present inquiry.

These are the equations which govern the distribution of stress; and it may be convenient to confer upon the ellipse whose radii vectores are in length inversely as the square roots of the pressures acting upon them, the name of the ellipse of *pressures*, in order to obviate any possibility of the position of this ellipse being confounded with that of the one which would, I believe, more ordinarily go by the name of the ellipse of *stress*. Every point in the mass is the centre of such an ellipse; and those ellipses, if properly drawn, will represent completely, and on the same scale, the magnitude and distribution of the pressures round about any point. It is almost needless to add that for a perfect fluid these ellipses would become circles.

Let us now proceed to establish the law of the variation of the stresses, or, to speak more accurately, of the thrusts acting on planes drawn in any given directions, on passing from one point of the mass to another. Returning to our little rectangular element PQRS, and considering the lines PQ, PS to be given in direction, so that we may consider $PQ = dx$ and $PS = dy$, and calling the units of pressure on PQ and RQ L and N, the unit of face-force M, the impressed forces of acceleration X in the direction of x , and Y in the direction of y , and the unit of mass ρ , by simple estimation of the forces in the directions of x and y respectively we obviously obtain, due attention being paid to the mode of fixing the positive directions of X and Y,

$$\frac{dL}{dy} + \frac{dM}{dx} = \rho Y,$$

$$\frac{dN}{dx} + \frac{dM}{dy} = \rho X.$$

If, as in the case with which we shall have to deal, the sole impressed force is that of gravity, and if we treat the weight of a unit of the mass as unity, and make the axis of x horizontal and of y vertical, the equations become

$$\frac{dL}{dy} + \frac{dM}{dx} = 1,$$

$$\frac{dN}{dx} + \frac{dM}{dy} = 0.$$

These, being the equations which control the law of the variation of the thrusts estimated in given directions in passing from one stress to another, I call the equations of variation of stress.

I now proceed to the application of the principles above set forth to the treatment of the particular question in hand.

Let μ be the coefficient of friction of the earth upon itself, and $\mu = \tan \lambda$, so that λ is the angle of repose; by this is to be understood that the thrust on any element can never make, with the perpendicular to that element, an angle greater than λ . Now the general law of the distribution of stress proves that the actual angle between the perpendicular to the element and its thrust will in two directions be zero. Hence at any given point it will pass through all gradations, from zero up to a certain limit. Here presents itself the question, Is that limit λ , or can it be λ for every point in the mass? As we have no right to assume *a priori* that this limiting angle in that state of equilibrium which we wish to determine must be equal to λ throughout the mass, and obviously it will not be so for actual cases of equilibrium which arise, we want a name to distinguish the maximum ratio which friction bears to pressure in any specified stress from the absolute maximum which this ratio is capable of attaining. We may name the former the coefficient of frictional energy; and for every point where this is equal to the absolute coefficient of friction, we may say the friction of the stress is at its maximum energy. Let (μ) be the coefficient of frictional energy for any given stress, and $(\lambda) = \tan^{-1} (\mu)$ the corresponding angle of repose. [We may also, if we please, term (μ) and (λ) the relative coefficient and relative angle of repose respectively, *i. e.* relative to any assigned stress.] Let the ratio between the maximum and minimum thrust of any stress be called γ^2 : a simple relation connects γ and (λ) *.

For calling, as before, L the pressure, and M the face-force (now the friction), we have by equation (1),

* This relation and its importance are well known to Professor Rankine.

$$L = P (\cos \theta)^2 + R (\sin \theta)^2,$$

$$M = (P - R) \sin \theta \cos \theta,$$

$$R = P\gamma^2,$$

$$\tan (\lambda) = (\mu) = \text{maximum value of } \frac{M}{L}.$$

To find this maximum, we have

$$\delta \{ \cot \theta + \gamma^2 \tan \theta \} = 0.$$

Hence

$$\gamma \tan \theta = 1,$$

and therefore

$$\cot (\lambda) = \frac{2\gamma}{1 - \gamma^2},$$

therefore

$$(1 - \gamma^2) - 2\gamma \tan \lambda = 0,$$

or

$$\gamma = \sec (\lambda) - \tan (\lambda) = \frac{1 - \sin (\lambda)}{\cos (\lambda)} = \tan \left(45 - \frac{(\lambda)}{2} \right).$$

This equation expresses the universal relation between the form of the ellipse of pressures for any stress and the relative angle of repose for such stress.

The problem we have just solved may be presented advantageously, in order to make the impression of it more vivid (as it is of cardinal importance), under a geometrical point of view. Taking any radius vector of the ellipse of pressures, the angle between it and its conjugate radius is 90° at any vertex ; at some point therefore it will be at a minimum, and this minimum will be the complement of the relative angle of repose.

From the preceding investigation, it will easily be seen that, to find the ray-directions which give this minimum, we have only to construct a rectangle circumscribing the ellipse, and either of its two diagonals will be in the direction required, and the angle between either such ray and the principal axes *plus* or *minus* half the angle between it and the normal (which angle is the relative angle of repose) will be half a right angle*.

* In fact the diameters which coincide with the directions of these diagonals are conjugate diameters, equally inclined to the principal axes ; and these, as I suppose must be well known, are the conjugate diameters whose inclination to each other is a minimum.

[To be continued.]

LXVII. *Experimental Researches on the Laws of Absorption of Liquids by Porous Substances.* By THOMAS TATE, Esq.

[Continued from p. 369.]

THE following experiment was made to show that, at equal distances from the surface of the liquid, the rate of diffusion is equal in all directions, that is to say, that the rate of diffusion is independent of the force of gravity.

Experiment IV.

The absorbent used in this experiment was unsized paper, 2 inches in width, cut longitudinally through the centre to within 1 inch of the lower extremity; and whilst one portion was suspended vertically, the other portion was bent after the manner shown in the annexed diagram. Each branch was graduated from the water-line of immersion, so that the space of ascent in the following Table is the distance traversed on the paper measured from the level surface of the water.



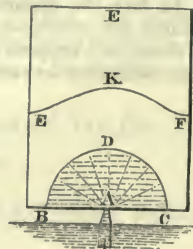
Ascent of liquid in inches, S.	On the vertical portion. Corresp. time in minutes, T.	On the bent portion. Corresp. time in minutes, T.	Value of T by formula $T = .9 S^2$.
0	0	0	0
1	.84	.80	.9
2	3.50	3.33	3.6
3	8.00	8.00	8.1
4	14.90	14.92	14.4
5	23.00	24.42	22.5

It will be observed how very nearly the results in the second and third columns coincide with each other, thereby establishing the law above enunciated.

The following experiment was made to test the truth of this law of equal diffusion.

Experiment V.

The absorbent used in this experiment was unsized paper, cut in the form represented in the annexed diagram. The width of the portion at A was as fine as possible. The paper was graduated from A along the vertical line A E, as well as from A along the horizontal line B C. The ascent recorded in the following Table is the vertical as well as the horizontal space traversed by the liquid on the paper estimated from the



point A, which stood about half an inch from the level of the liquid.

The line formed upon the surface of the absorbent by the liquid throughout the course of its diffusion, was a perfect semicircle, B D C, having the point A as its centre. The breadth of the absorbent being 3 inches, the semicircle attained its greatest diameter at $S=1\cdot5$ inch. After this the line of liquid absorption became a curve of contrary flexure, E K F, having points of contrary flexure at E and F, which may be mathematically explained on the assumption of the law of equal diffusion. The point A appeared to form the central point of liquid diffusion, from which the liquid was diffused equally in all directions.

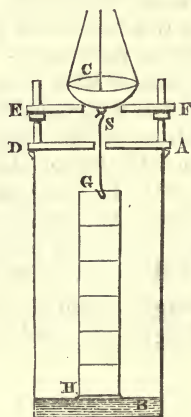
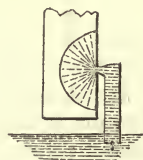
Ascent of liquid in inches, S.	Corresp. time in minutes, T.	Velocity of diffu- sion per minute, v.	Value of v by formula $v = \frac{1}{16 S}$.
0	0		
·4	1·23		
·5	1·83	·125	·125
·6	2·83		
·7	3·83		
·8	5·00	·080	·078
·9	6·33		
1·0	7·83	·060	·062
1·1	9·66		
1·4	15·58		
1·5	18·00	·041	·041
2·0	32·08		
3·0	72·00		

The same results were obtained with an absorbent cut in the form represented in the annexed diagram.

The following experiment was made to determine the manner in which the liquid diffuses itself over the surface of the absorbent.

Experiment VI.

This experiment was made with the apparatus represented in the annexed diagram. H G the absorbent suspended in the jar A B, containing a small portion of water, by means of a hooked wire, G S, attached to the pan C of a delicate balance; E F a stage supporting the pan of the balance: this stage admits of being raised or depressed as may be required, and the adjustment is such that the lower edge, H, of the absorbent shall exactly come in contact with the surface of the water,



B, whilst the pan rests upon the stage; A D a ground plate covering the top of the jar, having an orifice through its centre to allow the wire G S to pass freely through it.

The absorbent being carefully graduated from its lower edge, was suspended for some time in the humid air of the jar A B; the paper and hook, thus suspended, was balanced by weights placed in the opposite pan of the balance; the stage was then lowered until the edge, H, of the absorbent came in contact with the water. When the liquid had arrived at the different marks upon the absorbent, the beam of the balance was elevated so as to withdraw the absorbent from contact with the water; in this position the increased weight of the absorbent was determined, and the weight thus found was entered in the second column of the following Table. The numbers entered in the third column are the weights in grains requisite to break the contact of the absorbent with the water; and the numbers in the fourth column are the differences between the corresponding numbers in the second and third columns. The absorbent used in this experiment was unsized paper, 1.5 inch in width and 6 inches in length.

Ascent of liquid in inches, S.	Corresp. weight of liquid ab- sorbed in grains, w .	Corresp. weight to break contact, w_1 .	Weight of water of cohesion in grains, w_2 .	Value of w by formula $w=2.6\ S$.
1	2.63	2.6
2	5.22	5.2
3	7.80	15.08	7.28	7.8
4	10.40	17.60	7.20	10.4
5	12.93	20.14	7.21	13.0
6	15.44	22.64	7.20	15.6

It will be seen how nearly the value of w , obtained from the formula $w=2.6\ S$, coincides with the experimental results in the second column of the Table. Hence we conclude that *the liquid is equally diffused over the surface of the absorbent*.

As this result was not anticipated, its truth was tested in the following manner:—

A similar strip of unsized paper, 10 inches long and 1.5 inch wide, was suspended with its lower extremity in contact with the liquid, as in the foregoing experiment. When the liquid had reached the top of the absorbent, the augmentation of weight due to the liquid absorbed was determined. After the absorbent had been thoroughly dried it was cut transversely through the middle, and the process was repeated with this half portion. The following results were obtained:—

Weight of liquid absorbed by the 10-inch strip = 22.03 grs.

Weight of liquid absorbed by the 5-inch strip = 10.95 grs.

But the former result is almost exactly double the latter: hence it follows that the weight of water diffused through the upper half of the 10-inch absorbent must have been very nearly equal to the weight of water diffused through the lower half.

Let U = the work performed by absorption per minute.

k = the number of inches in one foot.

k_1 = the number of grains in one pound.

w' = the weight of water in grains diffused through each inch of the absorbent.

Then

$$T \times U = \frac{S}{2k} \times \frac{w}{k_1} + \left(\frac{v}{60k} \right)^2 \times \frac{w}{2gk_1}.$$

But $w = w'S$; $T = \alpha S^2$; and $v = \frac{1}{2\alpha S}$;

$$\therefore U = \frac{1}{\alpha} \cdot \frac{w'}{2kk_1} \left\{ 1 + \frac{1}{14400gk\alpha^2S^3} \right\}.$$

But for all appreciable values of S the fraction $\frac{1}{14400gk\alpha^2S^3}$ is indefinitely small as compared with 1;

$$\therefore U = \frac{1}{\alpha} \cdot \frac{w'}{2kk_1},$$

which expression, being independent of S and T , is the same for all points of ascent. Hence we infer that *in equal times the force of absorption performs the same amount of work.*

Experiment VII.

The absorbent used in this experiment was fine calico, 2 inches in width, and uniform in its texture. The temperature was 56° throughout the experiment. As in the preceding experiments, the liquid was distilled water.

Ascent of liquid in inches, S .	Corresp. time in minutes, T .	Value of T by formula $T = 1.12 S^2$.
0	0	0
1	1.12	1.12
2	4.50	4.48
3	9.50	10.08
4	17.33	17.92
5	27.00	28.00
6	40.00	40.32
7	56.50	54.88

In this case the velocity of ascent is expressed by the formula

$$v = \frac{1}{2.24S}.$$

In order to compare the absorbing powers of different absorbents, let f and f' be put for the velocities corresponding to the same time T of ascent of the liquid on two different absorbents, then we get from equation (2),

$$f = \frac{1}{2\sqrt{\alpha(T-\rho)}},$$

$$\therefore \frac{f}{f'} = \sqrt{\frac{\alpha'(T+\rho')}{\alpha(T+\rho)}}; \quad \dots \dots \dots (9)$$

and neglecting ρ and ρ' , we get

$$\frac{f}{f'} = \sqrt{\frac{\alpha'}{\alpha}}; \quad \dots \dots \dots (10)$$

that is, in this case the absorbing power varies inversely as the square root of α , or inversely as the square root of the time which the liquid takes to ascend one inch.

Now taking the absorbing power of the unsized paper of Exp. I. as unity, we find from equation (10),

$$\text{The absorbing power of the calico in the above experiment} \\ = \sqrt{\frac{3}{1.12}} = 1.63;$$

that is to say, the absorbing power of the calico is 1.63 times that of the unsized paper.

In this manner the powers of different absorbents may be definitely expressed.

These formulæ also give us the relative diffusibility of different liquids in any given absorbent.

Experiment VIII.

The liquid used in this experiment was turpentine, and the absorbents were unsized paper and calico. The temperature was 55° throughout the experiment.

A. Unsized paper of Experiment I.

B. Calico of Experiment VII.

Ascent of liquid in inches, S.	Unsized paper A.		Calico B.	
	Corresp. time in minutes, T.	Value of T by formula $T=3 S^2$.	Corresp. time in minutes, T.	Value of T by formula $T=1.7 S^2$.
0	0	0	0	0
1	2.9	3	1.6	1.7
2	12.0	12	6.5	6.8
3	27.0	27	14.6	15.3
4	50.3	48	26.5	27.2
5	80.0	75	43.0	42.5

The results of this experiment, on being compared with those of Experiments I. and VII., show that the diffusibility of turpentine on unsized paper is the same as that of water, and that its diffusibility on calico is somewhat less than that of water.

Taking the diffusibility of water on calico as unity, we get from equation (10),

$$\text{Diffusibility of turpentine on calico} = \sqrt{\frac{1.12}{1.7}} = .8124.$$

In like manner, taking the absorbing power of the unsized paper A in relation to turpentine as unity,

$$\text{The absorbing power of the calico} = \sqrt{\frac{2.9}{1.7}} = 1.3.$$

Experiment IX.

The absorbents used in this experiment were strips of linen and unsized paper. The liquids were distilled water, and a solution of starch containing 1 part of starch to 100 parts of water.

L. Linen absorbent weighing .8 gr. per square inch.

E. Unsized paper absorbent.

Ascent of liquid in inches, S.	Liquid. Solution of starch.				Liquid. Distilled water.			
	Absorbent L.		Absorbent E.		Absorbent L.		Absorbent E.	
	Corresp. time in minutes, T.	Value of T by formula $T = 2.5 S^2$.	Corresp. time in minutes, T.	Value of T by formula $T = 5 S^2$.	Corresp. time in minutes, T.	Value of T by formula $T = 1.5 S^2$.	Corresp. time in minutes, T.	Value of T by formula $T = 2.25 S^2$.
0	0	0	0	0	0	0	0	0
1	2.5	2.5	5.0	5	1.50	1.5	2.25	2.25
2	9.6	10.0	21.0	20	7.00	6.0	9.75	9.00
3	21.5	22.5	45.0	45	14.00	13.5	23.75	20.25
4	40.0	40.0	78.5	80	23.75	24.0	37.00	36.00
5	63.0	62.5	36.50	37.5	56.00	56.25

The results of this experiment show that the diffusibility of the solution of starch is very low as compared with that of water.

Taking unity as the diffusibility of water on each of the absorbents respectively, the diffusibility of the solution of starch on the paper absorbent will be expressed by $\sqrt{\frac{2.25}{5}} = .67$;

whereas on the calico absorbent it will be expressed by $\sqrt{\frac{1.5}{2.5}} = .77$. Whence it appears that the diffusibility of liquids varies with the nature of the absorbent. In comparing, however, the absorbing powers of two substances with each other, as a

general rule the substance which has the higher absorbing power for one liquid will also have the higher absorbing power for any other liquid.

Experiment X.

The liquid used in this experiment was linseed oil, and the absorbent was unsized paper, whose absorbing power for water at 60° is expressed by the formula $T=3S^2$. The experiment was made at two different temperatures.

Ascent of liquid in inches, S.	At the temperature of 60° .		At the temperature of 85° .	
	Corresp. time in minutes, T.	Value of T by formula $T=64S^2$.	Corresp. time in minutes, T.	Value of T by formula $T=48S^2$.
0	0	0	0	0
.25	3	3
.50	16	16	12	12
.75	26	27
1.00	65	64	46	48
1.25	73	75
1.50	147	144		
2.00	261	256		

Taking the diffusibility of water as unity, that of the oil will be expressed by $\sqrt{\frac{3}{64}} = .216$.

The results of this experiment also show that the rate of diffusion is increased by an increase of temperature. Within certain limits, the diffusibility of different liquids seems to increase proportionably with an increase of temperature.

The following experiment was made to determine the diffusibility of different solutions of starch in water.

Experiment XI.

The first solution of starch contained $\frac{1}{2}$ per cent. of starch, the second 1 per cent., and the third 2 per cent.; that is, the weights of starch dissolved in the same weights of water were as the numbers in the geometrical progression $\frac{1}{2}$, 1, and 2. The absorbent used was unsized paper of high absorbent power.

The results of the experiment gave the following formulæ of ascent of the different liquids:—

	Formulæ of ascent.
Distilled water	$T=1.1S^2$
Solution No. 1	$T=2S^2$
Solution No. 2	$T=5.6S^2$
Solution No. 3	$T=16S^2$

Hence we find from equation (10), diffusibility of water being unity,

Diffusibility sol. No. 1 = $\cdot 740 = \cdot 74 \times \cdot 6^0$.

„ „ No. 2 = $\cdot 447 = \cdot 74 \times \cdot 6$ very nearly.

„ „ No. 3 = $\cdot 262 = \cdot 74 \times \cdot 6^2$ very nearly.

Here it will be seen that the numbers expressing the diffusibility of the different solutions are very nearly in geometrical progression.

Hence we derive the following law:—*If the per-centage of starch contained in three different solutions be expressed by an ascending geometrical progression, the diffusibility of the solutions will be expressed by a descending geometrical progression.*

The results of the following experiment are confirmatory of this law.

Experiment XII.

In this experiment the liquids used were six different solutions of carbonate of potassa, containing respectively $\frac{1}{4}$, $\frac{1}{2}$, 1, 2, 4, and 8 per cent. of the salt; that is, the weights of salt dissolved in equal parts of distilled water were as the numbers in the geometrical progression $\frac{1}{4}$, $\frac{1}{2}$, 1, 2, 4, and 8. The absorbent used was unsized paper.

The results of the experiment gave the following formulæ of ascent of the different liquids:—

	Formulæ of ascent.
Distilled water	$T = 3\cdot 5 S^2$
Solution No. 1	$T = 3\cdot 25 S^2$
„ No. 2	$T = 3\cdot 75 S^2$
„ No. 3	$T = 4 S^2$
„ No. 4	$T = 4\cdot 33 S^2$
„ No. 5	$T = 5 S^2$
„ No. 6	$T = 5\cdot 33 S^2$

Hence we find from equation (10), diffusibility of water being unity.

	Diffusibility by formula $1\cdot 038 \times \cdot 59^n$.
Diffusibility sol. No. 1 = $1\cdot 038$. .	$1\cdot 038$
„ „ No. 2 = $\cdot 978$. .	$\cdot 986$
„ „ No. 3 = $\cdot 935$. .	$\cdot 936$
„ „ No. 4 = $\cdot 898$. .	$\cdot 889$
„ „ No. 5 = $\cdot 838$. .	$\cdot 844$
„ „ No. 6 = $\cdot 810$. .	$\cdot 801$

Hence it appears that the numbers expressing the diffusiveness of the different solutions are very nearly in geometrical progression, thereby confirming the law enunciated in the foregoing experiment.

The diffusibility of solution No. 1 is a little greater than that of pure water. A slight addition of an alkaline salt to water seems to increase its diffusiveness.

The following experiment was made to determine how far the law of diffusion is affected by direct chemical action.

Experiment XIII.

The absorbent used in this experiment was unsized paper saturated with a solution of 8 per cent. of carbonate of potassa and then carefully dried, so that the paper was uniformly impregnated with the salt. The liquid was diluted sulphuric acid having a specific gravity of 1.013.

Ascent of liquid in inches, S.	Corresp. time in minutes, T.	Value of T by formula $T = 11 S^2$.
0	0	0
1.0	11	11.0
2.0	43	44.0
2.5	68	68.7
3.0	97	99.0

Hence it appears that the general formula, expressing the rate of diffusion, holds true in cases where there is a known decided chemical action. It would seem that the ascent of the liquid through the pores of an absorbent is in most cases attended by chemical action.

The diluted acid used in this experiment was found to have the same diffusibility as distilled water, the formula of diffusion being $T = 3.5 S^2$ at mean temperature. The slow rate of diffusion of the diluted sulphuric acid in this experiment is not a little remarkable. In most cases direct chemical action tends to accelerate the diffusion.

The following experiment was made to determine the amount of heat evolved by the process of absorption.

Experiment XIV.

The bulb of a thermometer was infolded with dry unsized paper, which extended about one inch below the bulb. The extremity of the paper was brought in contact with water placed in a small-mouthed bottle. The temperature of the water was the same as that of the air, viz. 56°. As the liquid ascended through the pores of the absorbent, the temperature, indicated by the thermometer, gradually rose until it reached 61°, giving a maximum rise of 5° due to the heat evolved by absorption.

The naked bulb of a thermometer was inserted in some dry plaster of paris placed in an evaporating dish. A sufficient quantity of water, having the same temperature as the air, viz 56°, was then poured into the dish; after the higher portion of the plaster of paris had become wet by absorption, the temperature,

indicated by the thermometer, rose to $66^{\circ}\cdot 5$, giving a maximum rise of $10^{\circ}\cdot 5$ due to the heat evolved by absorption.

This change of specific heat, which always takes place during the process of absorption, is doubtless indicative of chemical action.

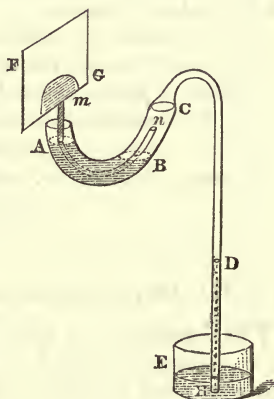
It may be worthy of remark, that an absorbent may be used again and again, with distilled water, without suffering any change in its power of absorption.

The following experiments were made to determine a statical measure of the force of absorption in different porous substances.

Experiment XV.

By means of the apparatus represented in the annexed diagram, the effect of the force of absorption in elevating a column of liquid may be strikingly exhibited.

A B C D a bent tube, open at both extremities, having the portion A B C considerably wider than the other portion C D K; the portion A B contains mercury, and that of B C water; F G a piece of dry calico, having a narrow strip, *m n*, proceeding from it, inserted through the mercury in contact with the water B C; the extremity K of the tube C D is inserted in a vessel E, containing water. As the absorbent, F G, draws the liquid from B C, the water rises in the tube K C

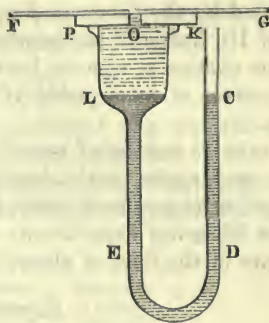


in opposition to gravity. If the diameter of the tube C D be $\frac{3}{16}$ ths of an inch, the water will have risen in the tube K C to the height of about 9 inches in the course of six hours, the air being in an average state of dryness; and when the air is unusually warm and dry, to a much greater height in that time. The effect is simply due to the force of absorption, there being no endosmose connected with the phenomenon.

The following form of this apparatus enables us to measure the force of absorption by the fall of a column of mercury.

L E D C a small bent tube proceeding from a wide tube or cup L P, ground at the top, which is covered by a plate, P K, of polished slate, having one or more small perforations, O, through it; F G an absorbent placed upon the plate. The bent portion L E D C contains mercury, and the cup, L P, is filled with water. As the liquid diffuses itself through the pores

of the absorbent, the mercury rises in the cup to supply the place of the water thus absorbed, and the column C D descends. This goes on until the force of absorption is balanced by the downward pressure of the mercury in the tube L E, that is, by a column of mercury measured by the descent of the mercury in the tube C D. The following results were obtained by means of this apparatus:—



Unsize paper laid upon the plate caused the column of mercury C D to descend 2·3 inches, which is equivalent to a column of 31 inches of water.

Brick caused the column of mercury C D to descend 1·3 inches, which is equivalent to a column of 17 inches of water.

Plaster of paris, encased in a glass tube, caused the mercury to descend 2·7 inches, which is equivalent to a column of 36 inches of water. And so on to other absorbents.

Hastings, November 10, 1860.

[To be continued.]

LXVIII. *On the Conduction of Heat by Gases.* By G. MAGNUS*.

THE cooling of a body *in vacuo* depends simply on the exchange of heat by radiation between the cooling mass and the encircling envelope. If the space contains gas, an ascending current is formed, which accelerates the cooling, added to which the property which the gas has of transmitting heat, or its diathermancy, concurs in producing cooling, provided the gases can conduct heat. Dulong and Petit, in enunciating their law of the loss of heat, have neglected the last two actions, manifestly because they are infinitely small compared with the influence of the ascending currents. Since then it has been universally admitted that the differences in the cooling of different gases depend on the different mobility of their particles. Cooling takes place much more rapidly in hydrogen than in other gases. With the same amount of heat this gas expands not more, but less than atmospheric air; the changes in density in the former gas are less than in the latter. But it is the difference of specific gravity which produces currents. If, therefore, different gases by contact with a warmer body all become equally heated, the currents in

* Translated from the *Bericht der Berliner Akademie*, 1860, p. 485.

those gases which have a greater coefficient of expansion must be greater than in the rest; for example, in carbonic acid more than in hydrogen. As this is not the case, it must either be assumed that the friction of the gaseous particles against each other is so great that the influence of the greater expansion is neutralized by it, which will with difficulty be admitted, or it must be assumed that gases by contact with a hot body become heated to a different extent. Such a difference in the degree of heat would take place if the gases had different capacities for heat; but as the specific heats of hydrogen and atmospheric air are the same, there remains no other explanation for the more rapid cooling in hydrogen than that this gas can transmit heat from particle to particle, in other words, can conduct it, and that it possesses this property in a higher degree than other gases. Its low density appeared to be in disaccordance with this idea, and it appeared necessary to decide by experiments how far it is founded.

The impulse to these experiments was given by a repetition of Mr. Grove's interesting observation, according to which a platinum wire is less strongly heated when surrounded by hydrogen than by atmospheric air, or another gas. In this repetition it was found that hydrogen exerted its preventive action even when a layer only 0.5 millim. thick surrounded the wire, and it was the same whether the tube containing it was in a horizontal or vertical position. In such a narrow tube, especially when it is horizontal, currents can scarcely occur; and when there are none, there remains no other explanation than that hydrogen conducts heat better than other bodies.

The simplest mode of ascertaining whether a gas conducts heat, consists in warming it from above, and observing the action on a thermometer placed within. It might be objected to this method, that, even with heating from above, currents in the gas might be formed, and that thereby the temperature indicated by the thermometer in various gases might be different without any difference in conductivity.

There is one method of testing this objection. For if, in fact, a gas can conduct heat, the temperature assumed by a thermometer in a space heated from above must be lower when the conducting substance is wanting than when it is present; that is, it must be lower *in vacuo* than in a space filled with air.

In order to ascertain whether this was the case, a glass apparatus was used, in which a thermometer, observable from without, was firmly fixed. It could be filled with different gases, and these could be variously dilated. The upper part of this apparatus was maintained at the same temperature, namely that of boiling water, and the temperature was observed which a

thermometer introduced into the interior ultimately assumed. Of course the experiments with this apparatus were not made without numerous precautions; it was more particularly necessary that the whole apparatus should be always under the same conditions, so as to give off the heat imparted to it always in the same manner. For this it was necessary that the space surrounding it should always be at the same temperature. In these experiments the temperature of the surrounding space was 15 degrees.

In this way the following results were obtained:—

1. The temperature which a thermometer ultimately assumes in a space heated from above, differs when this space is filled with different gases.

2. In hydrogen the temperature is higher than in any other gas.

3. In this gas the temperature is higher than *in vacuo*; and the denser the gas is, the higher is the temperature.

4. Hence hydrogen conducts heat like metals.

5. In all other gases the temperature is lower than *in vacuo*; and the denser they are, the lower is the temperature.

6. It cannot hence be concluded that gases do not conduct heat, but only that they do this in so small a degree that the action of conduction is cancelled by their diathermancy.

7. This remarkable property of hydrogen is evinced not only when it moves freely, but also when it is contained between eider down, or any loose substance which hinders its motion.

8. The great conductibility of this gas is a further confirmation of its analogy with metals.

9. Hydrogen conducts not only heat, but also electricity, better than other gases.

LXIX. On a Relation between two Ternary Cubic Forms.

By A. CAYLEY, Esq.*

THE cubic form

$$x^3 + y^3 + z^3 + 6lxyz$$

is in general linearly transformable into the form

$$(X + Y + Z)^3 + 27kXYZ.$$

In fact, writing

$$X = 2lx - y - z,$$

$$Y = 2ly - z - x,$$

$$Z = 2lz - x - y,$$

* Communicated by the Author.

we have identically

$$(1-2l+4l^2)(X+Y+Z)^3+24(l-1)^3XYZ \\ =8(2l+1)^2(l-1)^3(x^3+y^3+z^3+6xyz);$$

and the value of k consequently is

$$k=-\frac{8(l-1)^3}{9(1-2l+4l^2)}.$$

If, however, $l=1$ or $l=-\frac{1}{2}$, the transformation fails. In the former case, viz. for $l=1$, the equations for the linear transformation become

$$X=2x-y-z,$$

$$Y=2y-z-x,$$

$$Z=2z-x-y,$$

which give $X+Y+Z=0$, so that X, Y, Z are no longer independent; and the formula of transformation becomes

$$(X+Y+Z)^3=0.$$

It may be noticed that the invariant S of the form

$$x^3+y^3+z^3+6xyz$$

is $S=-l+l^4$, so that $l=1$ is one of the values which make S vanish. And the above transformation is not applicable to the cubic form $x^3+y^3+z^3+6xyz$, which is a form for which S vanishes. The transformation, however, holds good for $l=0$, which is another value which makes S vanish; or it does apply to the form $x^3+y^3+z^3$, for which S vanishes. The transformation, in fact, is

$$(X+Y+Z)^3+24XYZ=-8(x^3+y^3+z^3),$$

with the linear equations

$$X=-y-z,$$

$$Y=-z-x,$$

$$Z=-x-y.$$

The above two forms for which S vanishes, viz.

$$x^3+y^3+z^3+6xyz,$$

$$x^3+y^3+z^3,$$

are, notwithstanding, equivalent to each other, as appears by the identical equation

$$(x+y+z)^3+(x+\omega y+\omega^2 z)^3+(x+\omega^2 y+\omega z)^3 \\ =3(x^3+y^3+z^3+6xyz),$$

where ω is an imaginary cube root of unity. In the latter of the

two cases of failure, viz. for $l = -\frac{1}{2}$, the equations for the linear transformations are

$$X = Y = Z = -x - y - z;$$

so that X, Y, Z are not only not independent, but they are connected by *two* linear relations. And the formula of transformation becomes

$$(X + Y + Z)^3 - 27XYZ = 0,$$

which is, in fact, true in virtue of the equations $X = Y = Z$.

The two forms of equation,

$$x^3 + y^3 + z^3 + 6lxyz = 0,$$

$$(x + y + z)^3 + 27kxyz = 0,$$

represent each of them equally well a curve of the third order without a double point. In the first form the three real points of inflexion are given by $(x=0, y+z=0)$, $(y=0, z+x=0)$, $(z=0, x+y=0)$; or what is the same thing, the points in question are the intersections of the lines $x=0, y=0, z=0$ with the line $x+y+z=0$; or we have $x+y+z=0$ for the equation of the line through the three points of inflexion. And the equations of the tangents at the points of inflexion are

$$2lx - y - z = 0, \quad 2ly - z - x = 0, \quad 2lz - x - y = 0.$$

For the second form it is obvious that the points of inflexion are the intersections of the lines $x=0, y=0, z=0$ with the line $x+y+z=0$; and, moreover, that the lines $x=0, y=0, z=0$ are the tangents at the point of inflexion.

The first of the above-mentioned forms, however, cannot represent a curve with a double point. In fact the condition for its doing so would be $1 + 8l^3 = 0$; but when this condition is satisfied, the left-hand side breaks up into linear factors, and the equation represents, not a proper curve of the third order, but a system of three lines. The second form *can* represent a curve having a double point; viz. if $k = -1$, the curve will have a conjugate or isolated point at the point $x=y=z$. It is clear *à priori* that $(x=0, y=0, z=0$ being real lines) neither of the forms can represent a curve of the third order having a double point with two real branches through it, since in this case the curve has only one real point of inflexion.

I have elsewhere used the word "node" to denote a double point, and I take the opportunity of suggesting the employment of the words "crunode" (*crus*) and "acnode" (*acus*) to denote respectively a double point with two real branches through it, and a conjugate or isolated point.

LXX. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, Ph.D., F.C.S.

[Continued from p. 385.]

PROF. SCHRÖTTER has described* the occurrence of ozone in a specimen of fluor-spar which is found at Wulsendorf. It is a darkish-blue variety, and when scratched or rubbed in a mortar, it emits a powerful odour closely resembling that of bleaching powder. The phenomenon has long been known, and has been ascribed to the presence of chlorous acid.

On rubbing the mineral in a mortar, Schrötter observed that the odour emitted was exceedingly like that of ozone. When it was triturated in a mortar with iodide of potassium solution, the liquor became brown from the liberation of iodine, and when a piece of ozone test-paper was held over the mortar it became distinctly blue. This reaction might, however, have been produced either by a chlorine compound or by ozone. But when triturated with water or with alkalies, although a powerful smell was produced, no trace of a chlorine compound could be detected in the liquid.

When the mineral was triturated with chloride of sodium, chlorine was liberated, as was evident from the smell, and also by the fact that the reaction for chlorine was obtained when a plate moistened with nitrate of silver was held over the mixture. This must have been due to ozone, which has the property of expelling chlorine from its compounds; it is inconceivable that any oxygen compound of chlorine could have the property of liberating chlorine under these circumstances. Besides, the mineral, when triturated with pure wood-charcoal, emitted no odour, while, as a control experiment showed, the reverse is the case with hypochlorite of lime.

When the mineral was heated, a strong odour was perceived, which afterwards disappeared. Heated more strongly it lost its dark-blue colour, and appeared of a reddish-brown colour from sesquioxide of iron.

The following experiment was conclusive as to the absence of chlorine:—The anterior part of a piece of combustion tubing was filled with pieces of the mineral, while the rest of the tube contained pieces of porcelain. A potash apparatus containing iodide of potassium and starch was fitted to the posterior part of the tube. The part containing the mineral was gently warmed, and the rest of the tube was heated to redness, while at the same time purified air was passed through the tube. No liberation of iodine was observed in the potash apparatus. Had any one of the oxygen compounds of chlorine been present, in its passage over

* *Sitz.-Ber. der Wiener Akademie*, vol. xli.

the porcelain it would have been decomposed into oxygen and chlorine, which would have liberated some iodine, whereas ozone would at that high temperature be converted into ordinary oxygen, which would be without action on the iodide.

To determine the quantity of ozone, a given weight of the mineral was triturated in a mortar with iodide of potassium, and the liberated iodine estimated by means of hyposulphite of soda. In this way its quantity was found to amount to 0.02 per cent.

The ozone doubtless exists ready formed in the mineral, as the following experiment shows that fluor-spar can absorb ozone. 17 grms. of a specimen of fluor-spar, which by a preliminary experiment was found to contain 0.00011 per cent. of ozone, was exposed for six hours in a current of strongly ozonized air, and at the expiration of that time was found to contain 0.001 per cent. Pieces of pumice exposed in a similar manner to ozonized air were found to absorb ozone.

Millon has communicated the following observations on nitrification*, which in another form have been already made by Schönbein†. Under the conditions of high temperature, nitre is always produced with regularity, provided a humus product, an ammoniacal salt, and a mixture of earthy carbonates are present. The solid mass must further be constantly moistened and exposed to the air.

The necessity of the presence of humus is not at once apparent, and yet it gives a clue to the process. The alkaline humate which is formed energetically absorbs oxygen, and this oxidation is transferred to the ammonia. The part which humus plays is evident from the fact that it may be replaced by other oxidizable substances, such as phosphorus. When a stick of this substance is placed in a large glass globe and half covered with ammoniacal water, a slow combustion of the phosphorus soon commences and incites that of the ammonia. After some time nitrate of ammonia is found in the liquor. The ammonia in this experiment may be replaced by carbonate of ammonia, but not by the hydrochlorate or the sulphate. It appears, therefore, that nitrification takes place in the atmosphere between the emergent part of the phosphorus, the water, the air, and the ammonia.

Millon also shows, as had already been done by Tuttle‡, that copper can be similarly used to effect nitrification.

Ammonia is not the only substance which can be thus oxidized. When a stick of phosphorus is placed in a colourless solution of a manganese salt, it speedily becomes of a beautiful violet tint.

* *Comptes Rendus*, October 8.

† *Phil. Mag.* vol. xiii. p. 440.

‡ *Liebig's Annalen*, vol. ci. p. 283.

G. Rose* has communicated the commencement of a series of experiments on the circumstances under which the different forms of carbonate of lime (calc-spar, Arragonite, and chalk) are formed. When chloride of calcium was added to a fusing mixture of equivalents of carbonate of potash and carbonate of soda, it dissolved without effervescence; when a portion of the cooled mass was placed in water it gradually dissolved, leaving a pulverulent residue of carbonate of lime. Viewed under the microscope shortly after treatment with water, it was found to consist of small globules, which after twenty-four hours changed into well-formed individual or aggregated rhombohedra.

When another piece was thrown into hot water and boiled, it was seen to consist of microscopic prisms with occasional rhombohedra. This residue, on standing, gradually changed into aggregations of calc-spar.

The same results were obtained when calc-spar or oxalate of lime were added to the fusing mixture, except that in the latter case some carbonic oxide was given off.

As calc-spar had not been directly formed in these experiments, Rose, with Siemen's assistance, repeated Sir James Hall's celebrated experiment, by which marble is said to be obtained when chalk or compact limestone is heated in a closed vessel under pressure. Fine chalk was pressed in a musket-barrel which was hermetically sealed, and heated in a gas furnace, the temperature of which could be raised so as to melt large masses of platinum. During the experiment the barrel burst. On afterwards examining the contents, the chalk was agglomerated into a compact light-bluish mass, full of cracks. The experiment was repeated with small pieces of calc-spar, but the same result was obtained. It appears thence that these substances are not changed into distinctly crystallized calc-spar by being heated in a closed space. Rose considers that Sir James Hall thought the compacted mass to be crystalline marble.

When a solution of carbonate of lime in carbonic acid water was allowed to stand, a crust and a sediment gradually formed. The latter consisted of chalk, the former was found to be composed of very perfect microscopic rhombohedra.

When the liquid was placed on a hot stove, a disengagement of gas commenced, which lasted for six or eight hours. In this case the crust which formed consisted chiefly of acicular crystals of *Arragonite*, while the deposit was nothing but well-shaped rhombohedra.

When the solution was evaporated, prisms of *Arragonite* and rhombohedra, laminæ and plates of calc-spar were obtained.

Hence by evaporating a solution of carbonate of lime in car-

* *Ber. der Berl. Akad.* 1860.

bonic acid at ordinary or higher temperatures, all the three forms are obtained—chalk, calc-spar, and Arragonite.

Calc-spar is formed under other circumstances. When a solution of bicarbonate of soda was precipitated with chloride of calcium, and the milky liquid allowed to stand, rhombohedra without Arragonite were obtained; but when chloride of calcium was precipitated by *neutral* carbonate of soda, Arragonite only was obtained. It appeared as if carbonate of lime only separated as calc-spar when surrounded by an atmosphere of carbonic acid.

To determine the limits within which the two forms are produced, the fused mass was gradually added to water at different temperatures. The result was, that between 100° and 90° Arragonite was almost exclusively formed: below that point, principally prisms of Arragonite and rhombohedra of calc-spar; at 70° the rhombohedra predominated, and laminæ of calc-spar appeared, and at 30° the formation of Arragonite ceased. The limit to its formation appeared to lie between 50° and 30° .

The author is still engaged with the subject.

Carius has described* a new method of elementary analysis, by which sulphur, phosphorus, and chlorine in organic bodies may be determined. It consists in heating the substance in closed tubes with nitric acid.

The substance to be analysed is filled into small thin glass bulbs which are sealed before the blowpipe. The bulb is then introduced into a tube of about half an inch diameter sealed at one end, and half-filled with nitric acid of sp. gr. 1.2. This tube is then drawn out in the gas-flame, and, the nitric acid having been raised to boiling, the tube is sealed. By careful management the bulb is broken, and the tube then heated in a Bunsen's air-bath. This consists of a rectangular sheet-iron or copper box, about 14 inches long and 4 inches high; in one end there are two openings, which contain pieces of iron gas-pipe about an inch wide and closed at one end. The tube to be heated is placed in one of these pipes and loosely closed by a cork. The bath is then heated by a Bunsen's burner, the temperature being indicated by a thermometer. The advantage of this air-bath is, that any possible explosion is quite harmless, provided the bath be placed with the open end of the iron tube in the corner of a room.

After the oxidation is complete, which occupies a few hours, the finely drawn out end of the tube is softened in the blowpipe flame; it bursts and gives exit to the gaseous products without any loss of the liquid. The sulphur is present in the liquid con-

* Liebig's *Annalen*, October 1860.

tents of the tube as sulphuric acid, and is determined in the usual way, with obvious analytical precautions.

The applicability of this method was shown by the analysis of bisulphide of carbon, of sulphite of ethyle, and of many other sulphur compounds, and gave very satisfactory results.

Some sulphur compounds by oxidation with nitric acid yield the sulphur in the form of sulphurous acid. These are best determined by fusion with nitrate of soda.

With chlorinated bodies, a part of the chlorine is usually found in the free state among the gases, and a modification of the method is necessary. This consists in opening the tube under a solution of sulphite of soda, by which the chlorine is absorbed and immediately converted into hydrochloric acid, which is determined in the usual manner.

Bromine and iodine can also be estimated by this method, which is further applicable to the case of metallic sulphides. The analytical results were found in all cases to be very exact.

The following experiments were undertaken by Caron* to ascertain the nature of the process of cementation.

An iron bar, completely surrounded by pieces of charcoal, was packed in a porcelain tube which was placed in a reverberatory furnace, and heated to redness, while pure hydrogen, carbonic oxide, nitrogen, air, and carburetted hydrogen gases were passed through the tube in successive operations, each lasting two hours. In none of these cases was there any true cementation.

With ammonia it was different; after two hours' heating, the bar was immediately tempered and hammered, and again tempered, and then exhibited a regular and beautiful cementation of $\frac{1}{16}$ inch in depth. This was attributable to the action of ammonia on carbon, forming at this temperature cyanide of ammonium, which gives up carbon to the iron and forms steel. A direct experiment was made, omitting the charcoal, and heating an iron bar placed in a porcelain tube to redness in a current of gaseous cyanide of ammonium. After two hours' heating, the bar was treated as before, and was found quite cemented, especially at the end nearest the place at which the gas entered.

It seemed probable that this property of cementation was not confined to cyanide of ammonium, but was shared by other alkaline cyanides: the cementation by means of yellow prussiate of potash is probably of this kind. To decide this point experimentally, the bar was placed in the tube surrounded by charcoal impregnated with carbonate of potash, and heated to redness in a current of air. Under these circumstances, as is well known, cyanide of potassium is formed. After two hours the bar was

* *Comptes Rendus*, October 8, 1860.

found cemented in a magnificent manner to a depth of $\frac{1}{10}$ inch. Soda, baryta, and strontia act in the same manner, but this is not the case with lime. This doubtless arises from the fact (which was proved by experiment) that it does not form cyanide of calcium when heated to redness in a current of cyanide of ammonium.

The action of the various receipts for cementation may be explained by the formation of cyanides. It will be found that in all cases they contain the elements of the formation of alkaline cyanides.

The production of tartaric acid by the oxidation of milk-sugar, which Liebig discovered, has been already noticed in this Journal*. Carlet† has investigated the action of nitric acid on dulcine, a saccharine substance resembling mannite, which is obtained from Madagascar. He employed, with some modifications, Liebig's process. The oxidation of dulcine had been previously studied by Laurent and Jacquelin, who obtained mucic and oxalic acids. Besides these, Carlet found about $1\frac{1}{2}$ to 2 per cent. tartaric acid, and a saccharine substance which appears to have the formula $C^{12}H^{12}O^{12}$; it differs from dulcine in not being coloured yellow by potash, and in reducing oxide of bismuth, which dulcine does not do.

In addition to these, Carlet ascertained the existence of *racemic acid* in quantities of about $1\frac{1}{2}$ per cent. of the dulcine taken. The crystals of this acid were found to be identical with those prepared in other ways, both in form and in chemical properties. A more striking proof was, that it was optically inactive, but could be transformed into dextro-tartaric acid and lævo-tartaric acid. This was effected under Pasteur's cooperation by preparing racemate of cinchonidine and exposing it to crystallization. After some days lævo-tartrate crystallized out, and afterwards dextro-tartrate, from which the respective acids were obtained. The great interest of this lies in the fact that dulcine, an optically inactive substance, gives rise to a substance which, though also inactive, can be decomposed into two bodies which are capable of deviating the plane of polarization in opposite directions to an equal extent. In the case of Liebig's experiment, tartaric acid, which is a right-polarizing substance, is obtained from milk-sugar, which is also right-polarizing. With reference to dulcine, two hypotheses may be made—either that an optically active substance can be obtained from an optically inactive substance, or, which is more probable, that dulcine is only apparently inactive, and is really composed of two substances, one right-polarizing and the other left-polarizing, which just

* Phil. Mag. vol. xviii. p. 483.

† *Comptes Rendus*, vol. li. p. 37.

neutralize one another. This is probably the case with many other substances: they are in the same condition as racemic acid, which was supposed to be inactive, until Pasteur showed the reverse.

Among the oxidation products of gelatine and albuminous substances, Schlieper observed the presence of an oily body with an odour like that of cinnamic aldehyde. It occurs along with hydrocyanic acid, acetonitrile, propionitrile, valeronitrile, and hydride of benzoyle. It was also found by Gückelberger among the oxidation products of fibrine, albumen, and caseine. Fröhde has investigated this body*, and has found it to be an aldehyde, which by oxidation is converted into a new aromatic acid.

He obtained it in greatest quantity by the oxidation of gelatine by a mixture of sulphuric acid and bichromate of potass; the distillate from this action was neutralized with carbonate of soda, and the liquid evaporated to dryness: the soda salts were decomposed with sulphuric acid, and the liberated solid acids freed from the accompanying volatile fatty acids by filtration and by crystallization. The solid acids contained the new acid; it was separated from benzoic acid, which accompanies it, by treatment with hot water, in which it is insoluble, while benzoic acid readily dissolves. The quantity was found to be about $\frac{1}{10}$ per cent. of the gelatine taken, that of benzoic acid being $\frac{1}{3}$ per cent.

The melting-point of the new acid is 97° , and it solidifies at $93-94^{\circ}$. It is readily soluble in ammonia and in ether. It expels carbonic acid from carbonates; ignited, it burns with a smoky flame like benzoic acid. It has the formula $C^{12}H^4O^4$, and the author names it *collinic acid*. Its neutral silver salt has the formula $C^{12}H^3AgO^4$; and there is a basic salt, $C^{12}H^3AgO^4, AgO$. The baryta salt has the formula $C^{12}H^3BaO^4 + HO$. The acid yields with iron a reddish-yellow precipitate.

The aldehyde of collinic acid is a viscous colourless liquid, which by absorption of oxygen assumes a golden-yellow colour. When digested with a strong solution of ammonia, it is converted into a crystalline body, which is apparently $\left. \begin{matrix} (C^{12}H^3)^3 \\ H^3 \end{matrix} \right\} N^2$, corresponding to hydrobenzamide.

Kolbe has given a preliminary notice† of some interesting researches which have been made in his laboratory.

Vogt investigated the action of reducing agents on the body $(C^{12}H^5)(S^2O^4)Cl$, in the expectation of replacing chlorine by

* *Journal für Praktische Chemie*, vol. lxxx. p. 344.

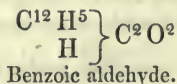
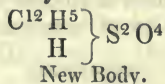
† Liebig's *Annalen*, September 1860.

hydrogen, and so forming a body $\left. \begin{smallmatrix} \text{C}^{12} \text{H}^5 \\ \text{H} \end{smallmatrix} \right\} \text{S}^2 \text{O}^4$, which, on Kolbe's view, would be an aldehyde derived from sulphuric acid. He found that the chloride was in fact reduced, but that the reduction went further, and yielded a new body, $\text{C}^{12} \text{H}^6 \text{S}^2$. This is a volatile stinking liquid, resembling mercaptan, and which, in contact with oxide of mercury, forms a body, $\text{C}^{12} \text{H}^5 \text{HgS}^2$.

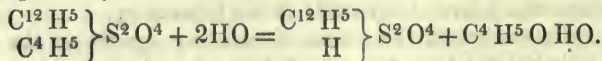
This body is, in fact, the mercaptan of an alcohol, $\text{C}^{12} \text{H}^6 \text{O}^2$, which is the next lower homologue of the alcohol of benzoic acid, $\text{C}^{14} \text{H}^8 \text{O}^2$. This latter body is usually called tolylic alcohol by continental chemists, and the hitherto hypothetical alcohol, $\text{C}^{12} \text{H}^6 \text{O}^2$, which is isomeric with phenylic alcohol, is called benzylic alcohol.

By treating the new sulphur-alcohol with pentachloride of phosphorus, Vogt obtained a chloride with an odour like chloride of toluyle, and which is probably chloride of benzyle, $\text{C}^{12} \text{H}^5 \text{Cl}$; from this he hopes to get the alcohol $\text{C}^{12} \text{H}^6 \text{O}^2$, and the acid $\text{C}^{12} \text{H}^4 \text{O}^4$. This latter, it may be observed, would be isomeric, and probably identical with Fröhde's new acid.

Kalle had investigated the action of chloride of benzoyl on zinc-ethyle, and had obtained a body, $\text{C}^{18} \text{H}^{10} \text{O}^2$, which was a mixed acetone, $\left. \begin{smallmatrix} \text{C}^{12} \text{H}^5 \\ \text{C}^4 \text{H}^5 \end{smallmatrix} \right\} \text{C}^2 \text{O}^2$. The publication of Freund's researches caused a discontinuance of these experiments. He tried the action of zinc-ethyle on the body $(\text{C}^{12} \text{H}^5) (\text{S}^2 \text{O}^4) \text{Cl}$, by which he hoped to obtain a body of the nature of an acetone, but containing sulphur, $\left. \begin{smallmatrix} \text{C}^{12} \text{H}^5 \\ \text{C}^4 \text{H}^5 \end{smallmatrix} \right\} \text{S}^2 \text{O}^4$. The two bodies, in fact, readily mix and form a solid white mass, which appears to be a compound of chloride of zinc and the new body. When this mixture is dissolved in water and evaporated, a new substance is obtained. This is not the above body: it has the formula $\text{C}^{12} \text{H}^6 \text{S}^2 \text{O}^4$, and Kolbe regards it as an aldehyde analogous to hydride of benzoyl:



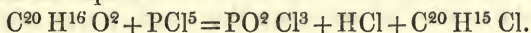
Its formation might be represented by supposing that the acetone had first been formed, and that then this body had been decomposed by the action of water. Thus:



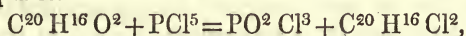
Pfaundler has examined the action of pentachloride of phosphorus on camphor*. When equivalents of these substances

* Liebig's *Annalen*, July 1860.

were mixed, the mass assumed a crumbly appearance: there was no action in the cold; but when heated to 60° a copious disengagement of hydrochloric acid gas took place, and a yellowish liquid was formed which commenced to boil at 83° . When this liquid was treated with water, a white flaky substance was deposited which crystallized from alcohol in beautiful feathery interwoven crystals. These, on analysis, were found to have the formula $\text{C}^{20} \text{H}^{15} \text{Cl}$, and their formation was obviously in accordance with the equation



When two equivalents of pentachloride were used for one of camphor, the reaction was found to be essentially the same, but the mass required to be heated to 100° . On subsequent mixture with water a viscous oil separated, which only became solid after a few days. This mass, by suitable treatment, yielded the body $\text{C}^{20} \text{H}^{16} \text{Cl}^2$ in crystals like the first. Placed over sulphuric acid, it gave off hydrochloric acid, and produced the first compound. It is probable that its formation proceeds in accordance with the equation



and that, in consequence of the heat, a portion of the body $\text{C}^{20} \text{H}^{16} \text{Cl}^2$ is decomposed.

This latter chloride was treated with sodium-alcohol, acetate of silver, and with ammonia, but the experiments did not give definite results. The compound $\text{C}^{20} \text{H}^{15} \text{Cl}$ does not act on the plane of polarization. The other produces a small right deviation.

LXXI. *On the Sensitiveness of the Human Ear to the Pitch of Musical Notes.* By F. FESSEL*.

THE Conservatory of Music at Cologne has recently decided on adopting, as a standard, the new Parisian tuning-fork; and the Concert Society here intend introducing the new pitch at the next winter concert.

In consequence of my having expressed a wish to possess a tuning-fork precisely similar to that belonging to the Conservatory, the fork that had been procured from Paris was kindly placed at my disposal.

In tuning forks I invariably pursue Scheibler's plan, which has hitherto proved itself the only safe one. The fork to be tuned, before having its vibrations compared with the seconds' pendulum, is, as is well known, tuned as far as possible by ear. My instrument and the seconds' pendulum happening on this

* Translated from Poggendorff's *Annalen*, No. 9, 1860, by F. Guthrie,

occasion to be in different rooms for the sake of convenience, I naturally endeavoured to finish tuning my fork by ear only. In this, however, I found I could not succeed; and having investigated all the circumstances with the greatest care, I was led to the following remarkable conclusion.

I observed that a fork which I had tuned by holding it to my *right* ear while the standard was held to my *left*, when compared with the fork used for the exact pitch, made one vibration too many in the course of several seconds; while a fork tuned by being held to my *left* ear while the standard was held to my *right*, vibrated less rapidly than the other. The fork in accurate pitch gave the lower note. Consequently I hear all notes somewhat higher with my right ear than with my left.

I have since examined my musical friends, and I have not yet found one, even among part-musicians, whose ears are precisely alike in estimating the pitch of musical notes. By continued practice I am able to distinguish, by a simple experiment, with which ear anyone hears the highest*. In this experiment I have never yet failed. The person under examination holds a carefully tuned fork in each hand, and having sounded them simultaneously, he brings them successively the one to the right, the other to the left ear. I place my right ear at equal distances from both of his, my left being turned away and covered lightly with my hand.

In this position that ear of the person under examination near which the fork is held which seems to me to have the highest pitch, hears all sounds higher than the other. If the tuning-forks are exchanged, precisely the same phenomenon results with respect both to the person under examination and to the listener. As far as my present experience extends, most people (here in Cologne) hear higher with the right ear than with the left.

These experiments are so striking that no one has hitherto attempted to dispute them. Indeed I had secured myself against contradiction, by always requesting the gentlemen whose hearing I tested to state their own opinions distinctly before I acquainted them with my explanation of the phenomenon. This precaution seemed to me to be necessary, since no one could blame a musician for resisting the imputation that he heard differently with different ears. In the end all were extremely astonished. I pass over the various playful questions and remarks that have been made to me—"Whether for the future, before beginning a concert, the hearers are to be examined and a place allotted to each accordingly, where he may be able to hear with satisfaction,"—"Whether instrumentalists are to be separated

* Without their having told me anything on the subject.

into two divisions, one of which is to use the *A right* pitch, the other the *A left*," &c. I content myself with simply asserting the fact as I have found it.

The reason for this difference of hearing is probably that the external passage of the ear is set in vibration, like a speaking-trumpet, by the sounds that enter it, and that this vibration modifies the pitch of the entering sound more or less according to the form of the individual ear.

The supposition that the waves of sound, before impinging on the tympanum, have to pass through a thin film which covers it, is less probable, since such a film would of course be subject to change from time to time, and thus the whole phænomenon might be altered.

As may be supposed, I have not as yet been able to collect any information on this subject.

If, in measuring the number of vibrations of musical notes, the above circumstance has not been taken into account, some modest doubt of the accuracy of the results may not be altogether unreasonable.

Cologne, September 1860.

LXXII. *Notes to the Meditation on Poncelet's Theorem, including a Valuation of the two new Definite Integrals*

$$\int_0^{\frac{\pi}{2}} \frac{\log \cos \phi}{\sqrt{1-b^2(\cos \phi)^2}}, \quad \int_0^{\frac{\pi}{2}} \frac{\log (1+\sqrt{1-b^2(\cos \phi)^2})}{\sqrt{1-b^2(\cos \phi)^2}}.$$

By J. J. SYLVESTER, M.A., F.R.S., Professor of Mathematics in the Royal Military Academy, Woolwich*.

Note A.

THE method given in the October Number of the Magazine for approximately representing a quadratic surd by a rational fraction is equally applicable to a surd of any degree. To fix the ideas, suppose we wish to approximate in this manner to $\sqrt[3]{R}$.

If we assume N as the first approximation, and make

$$L = P + \sqrt[3]{R}, \quad M = P + \rho \sqrt[3]{R}, \quad N = P + \rho^2 \sqrt[3]{R},$$

where $\rho^3 = 1$, and write

* Communicated by the Author.

$$F_1 = L^i + M^i + N^i,$$

$$F_2 = L^i + \rho^2 M^i + \rho N^i,$$

$$F_3 = L^i + \rho M^i + \rho^2 N^i,$$

$$U_1 = \frac{F_1}{F_2} R^{\frac{1}{3}}, \quad U_2 = \frac{F_2}{F_3} R^{\frac{1}{3}}, \quad U_3 = \frac{F_3}{F_1} R^{\frac{1}{3}},$$

$$V_1 = \frac{F_1}{F_3} R^{\frac{1}{3}}, \quad V_2 = \frac{F_3}{F_1} R^{\frac{1}{3}}, \quad V_3 = \frac{F_1}{F_2} R^{\frac{1}{3}},$$

we may easily establish the following propositions, which indeed are almost self-evident:—

(1) Each U and V is a rational fraction.

(2) When $i = \infty$, each $U = R^{\frac{1}{3}}$, each $V = R^{\frac{2}{3}}$.

(3) For all finite values of i , $R^{\frac{1}{3}}$ is intermediate between the least and greatest U , and $R^{\frac{2}{3}}$ between the least and greatest V .

So in general if k is any prime number, we may form $(k-1)$ cycles, each cycle containing k fractions possessing precisely analogous properties as regards representing approximately and limiting the successive powers of $R^{\frac{1}{k}}$. By means of these formulæ [the theory of which might be extended to algebraic quantities of every order (in Abel's sense of the word)], we obtain a complete command over the integration of surd quantities in general as they may appear in any physical problem, being thereby enabled to represent the integrals, not merely arithmetically, but analytically (which is of much higher importance) by logarithmic and circular functions to any degree of accuracy that may be required, and with known assignable numerical limits of error.

Note B.

This note relates to the concluding paragraph of the long note at page 313 in the October Number of the Magazine. I find that the i th inferior limit to $F(c) - \log \frac{2}{b}$, when c differs indefinitely little from unity given by the method therein explained, is

$$\log \frac{2}{b} + \frac{2}{i} \sum_{k=\frac{i}{2}}^{k=1} \frac{\cos \frac{2k-1}{2k} \pi}{\sqrt{1 + \left(\sin \frac{2k-1}{2i} \pi \right)^2}} \cos^{-1} \left(\sin \frac{2k-1}{2i} \pi \right)^2,$$

and that the superior limit is

$$\log \frac{2}{\delta} + \frac{\pi}{2i} + \frac{2}{i} \sum_{k=\frac{1}{2}}^{\frac{k-1}{2}} \frac{\cos \frac{k\pi}{i}}{\sqrt{1 + \left(\sin \frac{k\pi}{i}\right)^2}} \cos^{-1} \left(\sin \frac{k\pi}{i} \right)^2.$$

When $i = \infty$ these limits of course come together, and the finite sums resolve themselves into the definite integral

$$\frac{2}{\pi} \int_0^{\frac{\pi}{2}} d\tau \frac{\cos \tau}{\sqrt{1 + (\sin \tau)^2}} \cos^{-1} (\sin \tau),$$

of which, therefore, the value must be $\log 2$. Hence, writing $(\sin \tau)^2 = \cos 2\theta$, we obtain

$$\int_0^{\frac{\pi}{4}} d\theta \frac{\theta \sin \theta}{\sqrt{\cos \theta}} = \frac{\log 2}{\sqrt{2}} \frac{\pi}{4}.$$

Note C.

It may be shown that any of the expressions for $N^{\frac{1}{k}}$ derived from making $i = \infty$ in the general formulæ given in Note A, are in fact tantamount to its representation as a definite integral of a very simple kind. I shall not go into the proof of this here; it may be sufficient to indicate that it depends upon the fact that the equation of infinite degree $(\phi x)^i + (\psi x)^i + (\Im x)^i + \&c.$, may be resolved into sets of factors of a known form. In the question before us, the function to be so resolved is the denominator of any one of the quantities analogous to U or V in Note A; and ϕx , ψx , $\Im x \dots$ become linear functions of x with imaginary coefficients. Its resolution into factors is rendered possible by the circumstance that only two of the quantities ϕ , ψ , $\Im \dots$ can bear a finite ratio to each other for any given value of x , and consequently all the roots of the equation

$$(\phi x)^i + (\psi x)^i + (\Im x)^i \dots = 0^*$$

are contained among the roots of the several binary equations

$$(\phi x)^i = (\psi x)^i, \quad (\phi x)^i = (\Im x)^i, \quad \&c. :$$

which are the roots of any one of these equations (as *ex. gr.* of the first) that belong to the *given* equation will be determined by the condition that they must make the norms of all the other functions (*ex. gr.* of $\Im x$) indefinitely small as compared with the norms of those two which appear in it (*ex. gr.* ϕx , ψx). In this manner, if the total number of the functions is k , supposing

* My friend M. Jordan, of the Ecole des Mines (author of a remarkable thesis on *groups*), has developed some interesting geometrical consequences arising out of the study of this equation, which I hope he may be induced to publish.

$\phi, \psi, \vartheta \dots$ to be all linear functions of x , each binomial equation out of its entire stock of i roots will contribute $\frac{i}{\frac{k-1}{2}}$ roots

available towards the solution of the given equation. Mr. Cayley has remarked to me the analogy between this determination and Newton's method of finding the form of the several parabolic equations $y=cx^\lambda$ which represent the branches of a given algebraical curve at its origin. In the equation to the given curve $c x^\lambda$ is to be substituted for y ; the terms will then all become powers of x (an infinitesimal) whose indices will be linear functions of λ ; every pair of them in turn is equated to zero, and of all the values of λ thus obtained only those will be preserved which cause the two equated linear functions of λ belonging to any given pair of terms to be less than all the others, and consequently the terms themselves (whose indices the linear functions are) infinitely greater than all the other terms.

Linear functions of a variable figure in both investigations, viz. in Newton's as indices of the same infinitesimal quantity, in mine as quantities whose infinite index is the same*; but the logic and mode of procedure (utterly unlike as are the questions in their origin and subject matter) is the same in either case.

Note D.

The remark contained in the preceding note, as to the effect of representing $N^{\frac{1}{k}}$ by an infinite rational fraction being identical with that of expressing it as a definite integral, combined with a consideration of the cause of the success of the particular method referred to in Note B, has led me to the investigation following, of the value of the complete elliptic function of the first species. As usual denoting it by $F(c)$, we have

$$\begin{aligned} F(c) &= \int_0^{\frac{\pi}{2}} d\theta \frac{1}{\sqrt{1-c^2(\sin \theta)^2}} \\ &= \frac{2}{\pi} \int_0^{\frac{\pi}{2}} d\theta \int_0^\infty dx \frac{\cos \theta}{1-c^2(\sin \theta)^2 + (\cos \theta)^2 x^2} \\ &= \frac{2}{\pi} \int_0^\infty dx I, \end{aligned}$$

* In a word, Newton's equation is an exponential one made up of nothings, mine an algebraical one made up of infinities.

where

$$I = \int_0^{\frac{\pi}{2}} d\theta \frac{\cos \theta}{(1+x^2) - (c^2+x^2)(\sin \theta)^2}$$

$$= \frac{1}{\sqrt{(1+x^2)(c^2+x^2)}} \{ \log(\sqrt{1+x^2} + \sqrt{c^2+x^2}) - \log \sqrt{1-c^2} \}.$$

Let $x = \tan \phi$, $b = \sqrt{1-c^2}$;
then

$$F(c) = \frac{2}{\pi} \int_0^{\frac{\pi}{2}} \frac{d\phi}{\sqrt{1-b^2(\cos \phi)^2}} \{ \log((\sec \phi) + \sqrt{(\sec \phi)^2 - b^2}) - \log b \}$$

$$= \frac{2}{\pi} \log \frac{1}{b} \cdot F(b) + \frac{2}{\pi} R,$$

where

$$R = \int_0^{\frac{\pi}{2}} \frac{d\phi}{\sqrt{1-b^2(\cos \phi)^2}} \log(\sec \phi + \sqrt{(\sec \phi)^2 - b^2})$$

$$= \int_{\frac{\pi}{2}}^0 d\phi \left\{ \frac{\log(\cos \phi)}{\sqrt{1-b^2(\cos \phi)^2}} \right\}$$

$$+ \int_0^{\frac{\pi}{2}} d\phi \frac{\log(1 + \sqrt{1-b^2(\cos \phi)^2})}{\sqrt{1-b^2(\cos \phi)^2}}.$$

It will presently appear that these two definite integrals are equal to one another!

$$\text{Let } V_{2r} = \int_{\frac{\pi}{2}}^0 (\cos \phi)^{2r} \log(\cos \phi).$$

Then we may easily establish the formula of reduction,

$$V_{2r} = \frac{2r-1}{2r} V_{2r-2} - \frac{1.3.5 \dots (2r-3)}{2.4.6 \dots (2r-2)} \frac{\pi}{2r} ;$$

and since (as is well known) $V_0 = \frac{\pi}{2} \log 2$, we have

$$V_2 = \frac{1}{2} \frac{\pi}{2} \left(\log 2 - \frac{1}{1.2} \right),$$

$$V_4 = \frac{1.3}{2.4} \frac{\pi}{2} \left(\log 2 - \frac{1}{1.2} - \frac{1}{3.4} \right),$$

$$V_6 = \frac{1.3.5}{2.4.6} \frac{\pi}{2} \left(\log 2 - \frac{1}{1.2} - \frac{1}{3.4} - \frac{1}{5.6} \right),$$

&c. = &c.

Hence, by expanding the denominator in a series proceeding according to powers of $(\cos \phi)^2$, it is readily seen that the first integral becomes

$$\frac{\pi}{2} \left\{ \log 2 + \left(\frac{1}{2} \right)^2 \left(\log 2 - \frac{1}{1 \cdot 2} \right) b^2 + \left(\frac{1 \cdot 3}{2 \cdot 4} \right)^2 \left(\log 2 - \frac{1}{1 \cdot 2} - \frac{1}{3 \cdot 4} \right) b^4 + \&c. \right\}$$

To find the second integral, we must obtain the general term in the expansion in a series of powers of t of $\frac{\log(1 + \sqrt{1-t^2})}{\sqrt{1-t^2}}$

(where t stands for $b \cos \phi$), *i. e.* of $\frac{1}{\sqrt{1-t^2}} \int dt \left(\frac{1}{t} - \frac{1}{t\sqrt{1-t^2}} \right)$,

say of $\phi(t) = \frac{1}{\sqrt{1-t^2}} \psi t$. Now

$$\begin{aligned} \left(\frac{d}{dt} \right)^2 ((1-t^2)\phi t + \int dt(t\phi t)) &= \left(\frac{d}{dt} \right)^2 \left(\sqrt{1-t^2} \psi t + \int dt \frac{t\psi t}{\sqrt{1-t^2}} \right) \\ &= \psi t \left\{ \left(\frac{d}{dt} \right)^2 \sqrt{1-t^2} + \frac{d}{dt} \frac{t}{\sqrt{1-t^2}} \right\} + \psi' t \left(\frac{-2t}{\sqrt{1-t^2}} + \frac{t}{\sqrt{1-t^2}} \right) + \sqrt{1-t^2} \psi'' t \\ &= -\frac{t}{\sqrt{1-t^2}} \psi' t + \sqrt{1-t^2} \psi'' t \\ &= -\frac{t}{\sqrt{1-t^2}} \left\{ \frac{1}{t} - \frac{1}{t\sqrt{1-t^2}} \right\} - \sqrt{1-t^2} \left\{ \frac{1}{t^2} + \frac{2t^2-1}{t^2(1-t^2)^{\frac{3}{2}}} \right\} \\ &= -\frac{1}{\sqrt{1-t^2}} - \frac{\sqrt{1-t^2}}{t^2} + \frac{1}{1-t^2} - \frac{2t^2-1}{t^2(1-t^2)} \\ &= \frac{-1}{t^2\sqrt{1-t^2}} + \frac{2}{t^2} \\ &= \frac{1}{t^2} - \frac{1}{2} - \frac{1 \cdot 3}{2 \cdot 4} t^2 - \frac{1 \cdot 3 \cdot 5}{2 \cdot 4 \cdot 6} t^4, \&c. \end{aligned}$$

Hence, writing

$$\log \frac{(1 + \sqrt{1-t^2})}{\sqrt{1-t^2}} = \log 2 + K_2 t^2 + \dots + K_{2i-2} t^{2i-2} + K_{2i} t^{2i} + \&c.,$$

and equating the coefficients of t^{2i} , we obtain

$$2i(2i-1)(K_{2i} - K_{2i-2}) + (2i-1)K_{2i-2} = -\frac{1 \cdot 3 \cdot 5 \dots (2i-1)}{2 \cdot 4 \cdot 6 \dots 2i},$$

i. e.

$$K_{2i} = \frac{2i-1}{2i} V_{2i-2} - \frac{1 \cdot 3 \cdot 5 \dots 2i-3}{2 \cdot 4 \cdot 6 \dots (i-2)2i}.$$

Thus

$$K_0 = \log 2,$$

$$K_2 = \frac{1}{2} \left(\log 2 - \frac{1}{1.2} \right),$$

$$K_4 = \frac{1.3}{2.4} \left(\log 2 - \frac{1}{1.2} - \frac{1}{3.4} \right),$$

$$\&c. = \&c.,$$

and consequently

$$\int_0^{\frac{\pi}{2}} d\phi \frac{\log (1 + \sqrt{1 - b^2 (\cos \phi)^2})}{\sqrt{1 - b^2 (\cos \phi)^2}}$$

$$= \frac{\pi}{2} \left\{ \log 2 + \left(\frac{1}{2} \right)^2 \left(\log 2 - \frac{1}{1.2} \right) b^2 + \left(\frac{1.3}{2.4} \right) \left(\log 2 - \frac{1}{1.2} - \frac{1}{3.4} \right) b^4 + \&c. \right\}.$$

Thus, then, we obtain the following remarkable equalities:

$$\frac{\pi}{2} F(c) = \log \frac{1}{b} F(b) + 2 \int_{\frac{\pi}{2}}^0 d\phi \frac{\log (\cos \phi)}{\sqrt{1 - b^2 (\cos \phi)^2}}$$

$$= \log \frac{1}{b} F(b) + 2 \int_0^{\frac{\pi}{2}} d\phi \frac{\log (1 + \sqrt{1 - b^2 (\cos \phi)^2})}{\sqrt{1 - b^2 (\cos \phi)^2}},$$

or

$$\int_0^{\frac{\pi}{2}} d\phi \frac{\log (1 + \sqrt{1 - b^2 (\cos \phi)^2})}{\sqrt{1 - b^2 (\cos \phi)^2}}$$

$$= \int_{\frac{\pi}{2}}^0 d\phi \frac{\log \cos \phi}{\sqrt{1 - b^2 (\cos \phi)^2}}$$

$$\left[= \int_0^{\frac{\pi}{2}} d\phi F(b, \phi) \cot \phi \right]$$

$$= \frac{\pi}{4} Fc + \frac{1}{2} \log b Fb.$$

When b is indefinitely small, it is obvious from either of these equations that

$$F(c) = -\frac{2}{\pi} \log b \frac{\pi}{2} + 2 \log 2 = \log \frac{4}{b},$$

Legendre's well-known formula previously referred to.

The equality of the first two definite integrals in the *sortes*

above given, is, as we have seen, a consequence of the equality

$$\frac{\log(1 + \sqrt{1-t^2})}{\sqrt{1-t^2}} = \frac{2}{\pi} \int_0^{\frac{\pi}{2}} d\theta \{ \log \cos \theta + \log \cos \theta (\cos \theta)^2 t^2 + \log \cos \theta (\cos \theta)^4 t^2 + \&c. \}.$$

Hence we have

$$\int_0^{\frac{\pi}{2}} \frac{\log \cos \theta}{1-b^2(\cos \theta)^2} d\theta = \frac{2}{\pi} \frac{\log(1 + \sqrt{1-b^2})}{\sqrt{1-b^2}}.*$$

The extreme facility and brevity with which the method in the text gives the value of $F(c)$ for b indefinitely small is worthy of notice, as in the usual text-books it is obtained by a very indirect and circuitous process. We may obtain in like manner the value of

$$\int_0^{\frac{\pi}{2}} d\theta \frac{1}{(1-e \sin \theta)} \cdot \frac{1}{\sqrt{1-c^2(\sin \theta)^2}}$$

on the same supposition as to c , whether $1-e$ vanishes with $(1-c)$ or remains finite when $c=1$. On the latter supposition, the definite integral in question has for its value

$$\frac{1}{1-e} \log \frac{2}{b} + \frac{1}{1-e^2} \log \frac{2}{(1+e)^e}.$$

When $e=1$, this becomes infinite; when $e=-1$, the second term becomes $\frac{1}{4} + \frac{1}{2} \log 2$, and the entire integral is $\frac{1}{2} \log \frac{4}{b} + \frac{1}{4}$; when $e=0$, it is $\log \frac{4}{b}$. Subtracting the half of the latter integral from the former, we shall obtain

$$\int_0^{\frac{\pi}{2}} d\theta \frac{(1 - \sin \theta)^2}{(\cos \theta)^3} = \frac{1}{2},$$

which is easily verified.

By taking successively $e=\sqrt{-n}$, $e=-\sqrt{-n}$, and adding together the halves of the two integrals corresponding to these suppositions, we obtain the *ultimate* value of the complete elliptic

* From this it will readily be seen that when n is any integer we may obtain $\int_0^{\frac{\pi}{2}} \frac{\log \cos \theta}{(1-b^2(\cos \theta)^2)^n} d\theta$ by processes of differentiation in a form involving only algebraical and logarithmic quantities, and so, from what precedes, when n is any half-integer, in terms of such quantities and of complete elliptic functions.

integral of the third kind, viz.

$$\int_0^{\frac{\pi}{2}} \frac{d\theta}{1+n(\sin \theta)^2} \cdot \frac{1}{\sqrt{1-c^2(\sin \theta)^2}},$$

from the general formula above given, always of course subject to the condition that c is supposed indefinitely near to 1*.

K, Woolwich Common,
November 1860.

LXXIII. Notices respecting New Books.

An Elementary Treatise on the Dynamics of a System of Rigid Bodies. With numerous Examples. By EDWARD JOHN ROUTH, M.A., Fellow and Assistant Tutor of St. Peter's College, Cambridge; Examiner in the University of London. Cambridge: Macmillan and Co. 1860.

MR. ROUTH was Senior Wrangler in 1854, and is already known as the joint author with Lord Brougham, of 'The Analytical View of Newton's Principia.' His present work is intended as a text-book for students at Cambridge, and will no doubt take its place as such. We notice in it several of the more recent methods of investigation, and such novelties, for instance, as the reference of the motion to 'moving axes,' which have not hitherto been incorporated into elementary books.

We should have been glad to have seen the analytical processes applied to such questions as the motion of a locomotive, and other *practical* problems, instead of the purely fictitious and useless cases which seem to be the peculiar delight of Cambridge mathematicians; *e. g.*, such as the following:—"A perfectly rough, circular, horizontal board is capable of revolving freely round a vertical axis through its centre. A man whose weight is equal to that of the board walks on and round it at the edge: when he has completed the circuit, what will be his position in space?" (P. 53.) We feel tempted to parody this, and ask, "When the student has completed his circuit round a hundred such questions as this, what will be his position in science?" We believe it is not long since the Board of Mathematical Studies at Cambridge received some very strong hints on this subject from one of the most distinguished men of science in England, whose own mathematical attainments have always been applied to objects of real use and practical importance, instead of being frittered away in trifling ingenuity.

* It seems to be expected of every pilgrim up the slopes of the mathematical Parnassus, that he will at some point or other of his journey sit down and invent a definite integral or two towards the increase of the common stock. The author of these notes has been somewhat late in acquitting himself of this debt of honour, but ventures to hope that the principal results contained in the text above may be thought not unworthy of a place in some future edition of that noble and sumptuous monument of Dutch learning, industry, and fine taste, the invaluable collection of definite integrals by M. Bierens de Haan.

LXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 483.]

March 15, 1860.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communications were read:—

“On the Light radiated by heated Bodies.” By Balfour Stewart, Esq., A.M.

In two papers read before the Royal Society of Edinburgh in the years 1858 and 1859, and published in their Transactions, I have described some experiments on radiant heat, which would seem to involve an extension of Prevost’s theory of radiation, known as the theory of exchanges.

As the paper which I have now the honour to submit to this Society will detail analogous experiments on radiant light, I may be permitted briefly to refer to those points in my previous papers which are thus intimately connected with the present subject.

In attempting to unfold the logical consequences of Prevost’s theory, certain properties of radiant heat present themselves to our view, many of which are capable of experimental verification.

The following are some of these; and, for convenience-sake, I shall follow up the statement of each (before proceeding to the next) with a description of the analogous property of radiant light, as in this way the similarity which exists between heat and light will be most readily perceived.

In the first place, the heat radiated by a thin plate of any substance at a given temperature, is proportional to the absorptive capacity of that substance for the heat of that temperature; or, in few words, its radiation is equal to its absorption.

Rock-salt, for instance, has a small absorptive capacity for heat of 212° F., and, in consequence, its radiation when heated to 212° F. is comparatively small. In point of fact, a plate of this substance, 0.18 inch thick, only gives out 15 per cent. of the heat which lamp-black radiates at the same temperature. Glass, on the other hand, absorbing nearly all the heat of 212° F. which falls upon it, has at this temperature a radiation comparatively great, and nearly equal to that of lamp-black. A similar law holds with regard to radiant light.

If a piece of perfectly transparent glass be heated in an ordinary fire, removed to the dark, and there viewed, it will be found to emit scarcely any light; if the glass be slightly coloured, its radiation will be more copious; the amount of light given out, as far as I have been able to make the comparison, invariably depending upon the depth of colour or absorptive power of the glass for light, provided its colour stands heating. A good way of performing this experiment is to heat a dark glass by the side of a colourless one, by means of a chemical tongs, in some uniform field of heat. When viewed in the dark together, the contrast is very striking between the bright light of the one and the bare visibility of the other.

A stratum of heated gas may likewise be instanced as a substance which neither absorbs nor emits light to a sensible extent ; and it has similar properties with respect to heat.

Let us now proceed to another consequence of Prevost's theory. It is well known, from Melloni's experiments, that thin plates of various substances have the property of sifting the heat which falls upon them ; they stop certain rays, and allow others to pass,—the heat stopped being of one description, and the heat passed of another. Now, it may be shown to flow from the theory of exchanges, that the heat radiated by a thin plate of any substance at a given temperature is precisely that description of heat which the plate absorbs when heat of that temperature is allowed to fall upon it. The heat which it absorbs being that kind of heat which has a difficulty in passing through it, if the heat which it radiates be of this description also, it follows that the heat given out by a plate of any substance will experience difficulty in passing through a screen of the same substance. This we find to be the case : thus, a plate of rock-salt 0·77 inch thick passes only 30 per cent. of the heat from a thin plate of rock-salt heated to 212° F., whereas it will pass 75 per cent. of heat from lamp-black at that temperature. The same thing holds with regard to glass. A thin plate of crown glass will only pass half as much heat from heated crown glass as from heated lamp-black. But this peculiar quality of the radiating plate is destroyed if we coat the side of it furthest from the screen with lamp-black ; it then behaves precisely as lamp-black alone would do. The reason of this is that the rays of heat given out by the glass are the equivalent of those which it absorbs from the lamp-black behind ; so that both together give out the same heat in quantity and quality as lamp-black would alone.

We have here also analogous properties of light. Let us take a number of differently coloured glasses. With respect to the light of an ordinary fire, these may be divided into two groups ; viz. those which redden, and those which whiten the fire when we look through them. The first group comprises red and orange glasses ; the second group green and blue glasses.

Glasses of the former group absorb the whiter, glasses of the latter group, the redder descriptions of light. We should therefore expect red and orange glasses to give out, when heated, a peculiarly white light, and green and blue glasses a peculiarly red light. A number of red and orange glasses have been found which fulfil this expectation. Among the reds, those coloured by gold, when removed from the fire and held in the dark, give out a milky-white, or even greenish light ; and the orange glasses used by photographers do the same. Other glasses, of a dingy red tint, give out, when heated, light slightly whiter than the ordinary light of their temperature ; while there are others in which I have not, by this somewhat rude method of experimenting, been able to detect a sensible peculiarity of tint : yet this is not to be wondered at, if it be remembered that the following is the method in which the experiment is made. The glass under examination is held in a tongs,

along with another glass, opaque or nearly so, which gives out light of the ordinary description. They are heated together in the same field of heat, and then viewed in the dark as they cool. During this process the tint of each changes, becoming somewhat redder as the temperature falls. A difference in the temperature of the two glasses might therefore cloak or even reverse the peculiar difference of tint which it is sought to establish, unless this is very marked. This difficulty would be got over if we could by any means compare glasses of different tints at precisely the same temperature in the dark together; but I have not yet succeeded in contriving an apparatus for this purpose.

With respect to glasses that whiten the fire when used as screens, these all, without exception, as far as I have tried, give out a red description of light; and this is peculiarly remarkable in some light-green glasses.

The following circumstance renders some glasses unfit for experiment. They absorb nearly all the red light of low temperature, and it is only when the light rises to a white that a notable proportion is allowed to pass. Now if the law be true that the radiation is equal to the absorption, these glasses should, at a low red heat, give out nearly the whole light belonging to their temperature, the same as if they were opaque. It is only therefore when we raise the temperature that we can expect any result in the way of peculiarity of tint; for it is only then that the glasses, as screens, allow a notable proportion of light to pass, and that of a peculiar character; but the glass has now assumed a pasty condition, which renders it unfit to work with. Those glasses, therefore, are to be preferred that allow a considerable proportion of all the kinds of light to pass: for, just as in heat the radiation is most peculiar in rock-salt, which absorbs but little heat; so in light the best results are obtained by glasses that absorb only a small proportion of the light that falls upon them. As a proof of this, I may mention that difference of tint is very noticeable in the process of spinning threads of coloured glass. The heated red glass thread has, when being spun, a pale green hue, while the green glass thread has, under the same circumstances, a decidedly reddish appearance. These threads do not absorb much light, but what they do absorb is of a peculiar kind.

It has been mentioned that a screen of glass is peculiarly opaque for heat from glass; but if the side of the radiating plate furthest from the screen be coated with lamp-black, its heat now passes the glass screen as readily as ordinary heat of that temperature. A similar fact is noticeable with regard to light. A red glass, which, when heated and viewed in the dark, gives out a greenish light, while in the fire scarcely appears to differ in tint from the surrounding coals; and the same fact holds for all coloured glasses. Ultimately they all appear to lose their colour in the fire as they approach in temperature the coals around them. This may be explained thus:—the red glass, for instance, still gives out its greenish light; but it passes red light from the coals behind it, in such a

manner that the light which it radiates precisely makes up for that which it absorbs; so that we have virtually a *coal* radiation coming partly *from* and partly *through* the glass.

Let us now consider Prevost's theory with regard to bodies of indefinite thickness. One of its consequences was experimentally discovered by Leslie; viz. that metals which are good reflectors of heat are very bad radiators. As a variety of this experiment, I have endeavoured to show that a powdered diathermanous body will radiate less than powdered bodies which are opaque for heat. Thus, if a plate of table-salt have one side blackened, the white side will radiate only 83 per cent. of that which the blackened side radiates at the temperature of 212° F. No such difference is observed in sugar, which, though white for light, is black for heat of 212° .

We have here also similar facts with regard to light. If a pot of red-hot lead or tin be carried to the dark, and the dross scummed aside by means of a red-hot iron ladle, the liquid metal momentarily disclosed will appear less luminous than the surrounding dross,—the difference being more observable in the case of tin, which has a higher reflecting power than lead. Also, if a piece of platinum, partly polished and partly tarnished, be held above a flame in a dark room, the tarnished portion will shine much more brilliantly than the polished. Again, if we take a china cup with a white and black pattern, and heat it to a white heat in the fire, while there we shall not perceive much difference between the white and black of the pattern; but if we bring it into a dark room, we shall perceive the black to shine much more brilliantly than the white. This reversal of the pattern presents a very curious appearance.

Finally, it is a consequence of Prevost's theory and an experimental fact, that opaque bodies, generally speaking, radiate the same description of heat at the same temperature. In like manner, the light which they radiate is of the same description at the same temperature; one body is not red while a second is yellow and a third white, but they are all either red or yellow, or white together.

An analogy has thus been established between radiant heat and light in certain of their properties. Now two opinions have been entertained with regard to light:—

1st. Some have regarded it as differing from radiant heat only in wave length.

2nd. Others have regarded the two as physically distinct, although possessing many properties in common. It has even been thought that some kinds of light have no heating effect on the bodies on which they fall.

I cannot but think that the facts just stated countenance the former opinion rather than the latter: for Prevost's theory consists of the three following hypotheses:—

1st. That if an enclosure of any kind be kept at a uniform temperature, any body placed within the enclosure, and surrounded by it on all sides, will ultimately attain that temperature.

2nd. That all bodies are constantly giving out radiant heat, at a rate depending upon their substance and temperature, but independent of the substance or temperature of the bodies that surround them.

3rd. And, consequently, that when a body is kept at a uniform temperature, it receives back just as much heat as it gives out.

From these three assumptions may be deduced all the facts that have been stated with regard to radiant heat; but in the argument it is essential that the rays under consideration shall have the property of heating the bodies on which they fall, and by which they are absorbed. If this be not granted, the argument fails. Now radiant light, or those rays only that affect the retina, have been found to possess properties analogous to those which radiant heat thus possesses in virtue of its departure lowering the temperature of the body which it leaves, and its absorption raising that of the body on which it falls. If, therefore, we suppose all kinds of radiant light to have the property of raising (however little) the temperature of the body by which they are absorbed, the facts that have been stated in this paper regarding light may be shown to be a natural consequence of Prevost's theory of exchanges; but if, on the other hand, we do not admit that all the kinds of radiant light given out by heated bodies possess this property, then in that case those facts cannot be explained by Prevost's theory, but they will require a new theory to account for them.

This circumstance induces me to think that all the descriptions of light radiated by heated bodies have the power of heating, more or less, those bodies by which they are absorbed. Viewing the matter in this light, I have constructed the following Table, in which the logical consequences of Prevost's theory are stated in the first column, while opposite these in the second column are detailed the different experiments which they serve to explain.

Table of the consequences of Prevost's theory, and the facts which they explain.

Consequences of Prevost's theory.

Facts which these consequences explain.

The radiation of a thin plate or particle is equal to its absorption, and that for every description of heat—that is to say, in quality as well as in quantity.

Those opaque bodies which reflect most, radiate least. Opaque bodies generally give out the same kind of rays at the same temperature: these words also express the known fact.

Rock-salt which absorbs little heat of 212° F., gives out little; while glass, which absorbs much, gives out much.

The heat radiated by rock-salt has great difficulty in passing through a screen of rock-salt—the heat radiated by glass in passing through a screen of glass.

Colourless glass, when heated, gives out little light, opaque glass a great deal. Red glass, which absorbs the greenish rays, gives out greenish rays; while green glass, which absorbs the red rays, gives out red rays.

When a plate of glass is coated on its further side with lamp-black, its heat is the same as lamp-black heat.

All coloured glasses appear to lose their colour in the fire.

Metals radiate little, both of heat and light. Table-salt, which is white for heat of 212°, radiates less than sugar, which is black. When a black and white china cup is heated in the fire and held in the dark, the black of the pattern is more luminous than the white.

In conclusion, I may be permitted to remark regarding these laws of light, that from their simple nature some of them may have been observed before, but I think they are now for the first time connected with a theory of radiation.

Supplement (added March 7, 1860).—Since writing the above, the law which asserts that the absorption of a thin plate or particle is proportional to its radiation for every description of light, has received a very beautiful confirmation.

In the *Philosophical Magazine* for this month, pages 194–196, Professor Stokes has noticed some very interesting experiments of M. Foucault, and also of Professor Kirchhoff. M. Foucault finds as the result of his experiments, “that the voltaic arc formed between charcoal poles presents us with a medium which emits the ray D of the solar spectrum on its own account, and which at the same time absorbs it when it comes from another quarter.” Professor Kirchhoff, again, finds as the result of his experiments on the spectra of coloured flames, “that coloured flames, in the spectra of which bright sharp lines present themselves, so weaken rays of the colour of these lines when such rays pass through the flames, that, in place of the bright lines, dark ones appear as soon as there is brought behind the flame a source of light of sufficient intensity, in the spectrum of which these lines are otherwise wanting.”

We thus see that the same media which in a heated state emit rays of a certain refrangibility in great abundance, have also the property of stopping these very rays when they fall upon them from another quarter, or, in other words, their absorption of such rays is proportional to their radiation of them.

Supplement (added March 8, 1860).—The following fact noticed by Professor Kirchhoff is also in accordance with the theory brought forward in this paper.

“The spectrum of the Drummond light,” he remarks, “contains, as a general rule, the two bright lines D, if the luminous spot of the cylinder of lime has not long been exposed to the white heat; if the cylinder remains unmoved these lines become weaker, and finally vanish altogether. If they have vanished, or only faintly appear, an alcohol flame into which salt has been put, and which is placed between the cylinder of lime and the slit, causes two dark lines of remarkable sharpness and fineness to show themselves in their stead; but the Drummond light requires, in order that the lines D should come out in it dark, a salt-flame of lower temperature. The flame of alcohol containing water is fitted for this, but the flame of Bunsen’s gas-lamp is not. With the latter the smallest mixture of common salt, as soon as it makes itself generally perceptible, causes the bright lines of sodium to show themselves.”

Now, when we heat a piece of ruby glass in the fire, we have an analogous phenomenon. As long as the ruby glass is of a lower temperature than the coals behind it, the light given out is of a red description, because the ruby glass stops the green: the green here

is precisely analogous therefore to the line D, which is stopped by an alcohol flame into which salt has been put. Should, however, the ruby glass be of a much higher temperature than the coals behind it, the greenish light which it radiates overpowers the red which it transmits, so that the light which reaches the eye is green more than red. This is precisely analogous to what is observed when a Bunsen's gas-lamp with a little salt is placed before the Drummond light, when the line D is no longer dark but bright.

In fact, the law, "the absorption of a particle is equal to its radiation, and that for every kind of light," only applies to the case where the temperature of the particle is equal to that of the source of the light which passes through the particle. If the temperature of the source of light be greater, one quality of light will predominate; if, on the other hand, the temperature of the particle be greater, another quality of light will predominate.

"On the Luminous Discharge of Voltaic Batteries, when examined in Carbonic Acid Vacua." By J. P. Gassiot, Esq., F.R.S.

On the 24th of May, 1859 (Proceedings, May 26, 1859), I communicated to the Royal Society a short notice of my having obtained the stratified discharge from a voltaic battery of 3520 elements charged with rain-water; and also with one of 400 elements charged with nitric and sulphuric acids, each cell of both batteries being insulated. I stated also that with the latter (as I had previously shown with the former), spark discharges passed between two terminal copper plates through the air, before the completion of the circuit.

The well-known luminous arc in air, as usually obtained from an extended series of the nitric-acid battery, was very brilliant, but from the small size of the porous cells (3 inches long, $\frac{1}{2}$ inch broad) containing the nitric acid, it was only tried by a momentary action. With the water-battery I have never been able to obtain a continuous discharge in air similar to the voltaic arc; whether from points or plates, the discharges from this battery are invariably in the form of minute, clearly defined, and separated sparks.

Although the water-battery consisted of nearly nine times as many metallic elements as the nitric-acid battery, it exhibited by the gold-leaf electroscope very little increased signs of tension.

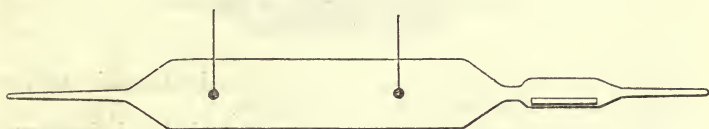
This is in accordance with the results obtained by me in 1844, when I showed that the tension of a single cell increased in force according to the chemical energy of the exciting liquid. "In all the experiments I made, the higher the chemical affinities of the elements used, the greater was the development of evidence of tension" (Philosophical Transactions, 1844, part 1. p. 52).

Recently, through the kind introduction of Professor Wheatstone, I have had an opportunity of experimenting with Daniell's constant battery; and my present object is to present to the Royal Society a short account of the results I have obtained, and to describe the appearance and character of the voltaic discharge of these several batteries when taken in vacua.

The vacuum-tubes I used were prepared by means of carbonic acid (*Philosophical Transactions*, 1859, part 1. p. 137). For the sake of reference I will denote these by the original numbers which I am accustomed to attach as the vacua are completed:—in the following experiments these were 146, 187, 190, 196, 202, and 219.

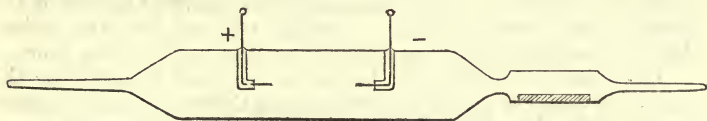
No. 146 is 24 inches long and 18 inches in circumference, and has a copper disc 4 inches diameter at one terminal and a brass wire at the other: this vessel is figured in my last communication. No. 187 and 196 (see fig. 1) are each about 6 inches long and 1 inch diameter, with gas-coke balls $\frac{1}{4}$ inch diameter, attached to the hermetically sealed platinum wires, the wires being protected by glass tubing as far as the balls, placed inside the tube 3 inches apart.

Fig. 1.



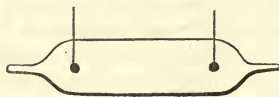
No. 190 has brass wires, and No. 202 silver wires attached to the platinum: both these tubes (fig. 2) are of the same dimensions as No. 187 and 196. No. 219 is 4 inches long, has gas-coke balls of

Fig. 2.



about $\frac{1}{3}$ th of an inch in diameter, and 1 inch apart: the caustic potash originally attached to this tube has been scaled off; the form is shown in fig. 3.

Fig. 3.



With the inductive coil the discharge in 146 exhibits a large cloud-like luminosity on the plate, which in these experiments was always made the negative terminal. On the positive wire, minute luminous spots were visible. At intervals, apparently by some sudden energetic action, flashes of bright stratified light would dart through the vacuum; but by carefully adjusting the contact breaker, the discharge could be made to pass, producing a white glow on the negative plate, without to the eye affording any appearance of an intermittent discharge.

In 187 and 196 the stratifications were narrow from the positive

terminal, the negative ball being surrounded with a narrow halo of light similar to that in fig. 1 A (p. 545), but smaller. In 190 there was a red tinge on the cloud-like discharge near the positive terminal; on the approach of a magnet, two or three additional clouds were brought out, the negative wire being covered with a luminous glow extending to the sides of the tube parallel to the end of the negative wire. 202 exhibited a remarkably well-defined cloud-like discharge, the several clouds being clearly and distinctly-separated. No. 219: the appearance of the discharge in this tube was similar to that in

Fig. 3 A.



fig. 3A, with a particularly brilliant glow around the negative ball, but without any stratifications from the positive.

Having thus described the appearances of the induced discharge in these tubes, I now proceed to the description of experiments made in the same vacua,—1st, with the water-battery of 3520 cells; 2ndly, with the 512 series of Daniell's constant battery; and lastly, with 400 series of Grove's nitric-acid battery.

With the water-battery, as I have stated in my previous communication, the stratified discharge, similar in character to that of the inductive coil, was obtained, not only in 146 and the other tubes, but also through 146 and any one of the others at the same time.

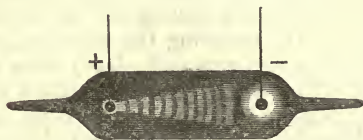
From the risk of fracture, I have not ventured again to heat the potash in 146, but I have invariably found that whenever the potash in any of the other tubes was heated, the discharge from the water-battery, instead of increasing in distinctness and brilliancy, entirely ceased.

The discharge from the water-battery through each of the tubes had the appearance of being continuous, and the needle of the galvanometer, placed on the circuit, showed a steady deflection. To test whether the discharge was continuous, I attached No. 219 to my rotating apparatus (*Philosophical Transactions*, 1859, part 1. p. 158); the discharges were then clearly separated, so that even with this apparatus they were shown to be intermittent.

As the water in the battery, after a lapse of some weeks, had partially evaporated, the action was so reduced that it would no longer pass through any of the vacuum-tubes except 219, in which it assumed the appearance in fig. 3 B. In this state it remained for three or four weeks; and whenever from change of temperature moisture was deposited on the surface of the glass tubes, the luminous discharge disappeared when the tubes were touched by the hand or by a wire, reappearing as the hand or wire was withdrawn. A portion of the discharge from this battery was evidently lost by insufficient insulation, reduced by accumulated dust and moisture. I have not the requisite

facilities at command, but a thoroughly well-insulated battery of 3000 or 4000 series would produce effects well worthy of examination.

Fig. 3 B.



Daniell's Constant Battery.

On the 27th of December, 1859, by the introduction of Professor Wheatstone, I had the opportunity of experimenting with the large battery belonging to the Telegraph Company at its Factory, Camden Town; Messrs. Wheatstone, Latimer Clarke, and Bartholomew were present. My object was to ascertain whether a luminous discharge could be obtained through the vacuum-tubes from the battery, which consists of 512 series of Daniell's elements, zinc and copper. The zinc plates are 2 by $3\frac{1}{2}$ inches, the copper $3\frac{1}{2}$ by $3\frac{1}{2}$ inches; the cells are insulated in series of 10 and 12, suspended on trays by gutta-percha bands. I experimented with three of my vacuum-tubes, Nos. 187, 196, and 219, with 187 and 196; no sign of any luminous discharge could be observed; the electroscopes (Peltier's and a gold-leaf), by the tension, showed that the current did not pass.

With 219 the brilliant glow around the negative ball, as is shown in the same tube by the induction coil, was visible with very trifling luminosity on the positive. I have attempted to show this appearance in fig. 3 A.

Various lengths, viz. 110 yards, 1, 2, 4, 8 and 16 miles of covered copper wire, the 110 yards covered with india-rubber, and the other lengths with gutta-percha, are kept immersed in a water-tank for the purpose of experiments at the factory; an opportunity was therefore offered of testing the action of the battery on these wires by means of vacuum-tubes. This immersed wire is designated the cable, acting in this respect in a manner analogous to submarine wires.

The general results obtained, and repeatedly verified, may be briefly stated as follows:—From the 110 yards to the greatest length of 16 miles, it took from half to one and one quarter of a second for the cable to receive as much of the charge of the whole battery as could pass through the vacuum-tube 219, the time being denoted by the appearance of the luminosity in the negative ball (see fig. 3 A).

With 110 yards the discharge of a charge previously given to the cable was instantaneous; it appeared to be nearly momentary with one mile, and the time then progressively increased according to the length of the cable previously charged, until with the 16 miles it took one and a quarter to one and a half seconds before the luminous glow on the ball in the vacuum-tube disappeared.

It was beautiful to see the regularity with which the glow appeared

and disappeared in these experiments, first at one terminal and then at the other, according as the cable was charged or discharged.

After the cable had received the discharge to the greatest intensity that could be obtained through the tube 219, the full charge of the battery was then completed by cutting off the tube from the circuit by means of a wire. On removing the wire, and substituting the earth for the battery, a discharge took place; but a residuary charge was always found, which could not pass through the vacuum. If this residuary was allowed to remain in the cable, and the battery again substituted for the earth, no additional charge could be made to pass from the battery through the vacuum-tube; but so soon as this residuary was discharged, the cable again became charged through the tube as before*.

We were particularly fortunate with these experiments; for on Mr. Wheatstone testing the capability or power of the battery, he ascertained that on taking from it only 32 cells, thus reducing the number to 480, the discharge could not pass through the vacuum-tube.

Grove's Nitric-Acid Battery.

Each set of elements in the battery were inserted in a glass vessel with a stem 6 inches long; the stems were carefully cleaned and dried. These precautions, with the high chemical affinity of the elements, raised the tension of each terminal, as denoted by gold-leaf electrosopes, to nearly that of the larger series of the water-battery. A succession of spark-discharges could be taken between the copper discs of my micrometer-electrometer, one disc being attached to the zinc, and the other to the platinum end of the battery.

In the following experiments, the different vacuum-tubes were introduced between one of the copper discs and the battery, as also a galvanometer. By this arrangement the circuit could be gradually completed without any risk of disarranging the apparatus, and the spark discharged obtained before the copper discs of the micrometer-electrometer came into contact.

"In 146, on the completion of the current, the discharge of the battery passed with a display of magnificent strata of most dazzling brightness. On separating the discs by means of a micrometer screw, the luminous discharges presented the same appearance as when taken from an induction coil, but brighter. On the copper plate in the vessel there was a white layer, and then a dark space about one inch broad; then a bluish atmosphere, curved like the

* If one of the wires of the vacuum-tube No. 219 is connected with the inner coating of a Leyden jar, and the other with the prime conductor of an electrical machine, when the machine is excited a luminous glow will be observed round one of the balls, similar to that obtained during the charging of the cable by the voltaic battery, and the jar will become gradually charged. On the excitation of the electrical machine being stopped, if a pointed wire is presented to the prime conductor, the jar will be gradually discharged, and the luminous glow appears on the *other* ball of the vacuum-tube; if several similar tubes are arranged in series, and the jar is discharged through longer carbonic-acid vacua, the striæ can be obtained in the latter.

plate, evidently three negative envelopes on a great scale. When the plate was positive, the effect was comparatively feeble." The preceding is an extract from notes made by the Rev. Dr. T. R. Robinson, when he first witnessed the experiments in my laboratory, on the 5th of August, 1859. With the same vacuum I have always obtained similar results.

In 187 and 196 (fig. 1) with carbon-balls in the tubes, the discharge of the nitric-acid battery elicits intense heat, and probably changes the condition of the vacuum. On the 5th of August, 1859, "the discharge in 187 presented a stream of light of intolerable brightness [I again quote from Dr. Robinson's notes], in which, through the plate of green glass, with which he observed the phenomena, strata could be observed. This soon changed to a sphere of light on the positive ball, which became red-hot, the negative being surrounded by magnificent envelopes, whilst with the horseshoe magnet the positive light was drawn out into strata. The needle of a galvanometer in circuit was violently deflected, and the polarity reversed, settling at a deflection of 45° . On heating the potassa, the discharge again bursts into a sun-like flame, subsequently subsiding into three or four large strata, of a cloud-like shape, but intensely bright."

On a subsequent occasion I found that the discharge did not pass for about a minute after the circuit was completed, when a fine glow appeared on the negative ball, fig. 1 A. Around the positive ball

Fig. 1 A.



there was a trifling glow, in a few seconds a momentary brilliant flash, and the discharge ceased. The potash was then again heated; the large negative glow reappeared, followed almost instantly by a remarkably brilliant stratified discharge, with intense chemical action in the battery, denoted by the evolution of nitrous fumes; at this moment I separated the discs of the micrometer-electrometer, and thus broke the circuit.

I now arranged the apparatus by attaching gold-leaf electrosopes to both terminals, and introduced the galvanometer, so as to enable me to examine more carefully the action that would take place when the potassa was heated. On heating the potassa, the fine negative glow (fig. 1 A) was again developed; the leaves of the electroscope did not close; but as the negative glow increased, the needle of the galvanometer was *suddenly* deflected, immediately (although the glow continued) returning to zero; as more heat was applied a small globe

of light appeared on the positive (fig. 1 B), and the needle *gradually* deflected 40° or 50° . On withdrawing the lamp, as the potash cooled, the positive glow disappeared, the needle of the galvano-

Fig. 1 B.



meter receded, the glow on the negative remaining more or less brilliant,—this action and reaction alternating as the heat of the lamp was applied or withdrawn from the potash.

When the heating of the potash was further increased, four or five cloud-like and remarkably clear strata came out from the positive (fig. 1 C); and these were quickly followed by a sudden discharge

Fig. 1 C.



of the most dazzling brightness, which remained for several seconds. The stratifications, which were conical in shape, I have endeavoured, although very faintly, to depict in fig. 1 D. The needle of the gal-

Fig. 1 D.



vanometer was suddenly and violently deflected, striking with considerable force the two corks placed to protect it on the compass card. At the instant this discharge took place, and not before, the leaves of the electroscopes collapsed. This, with the intense chemical action observable in the battery, proved that the entire current was passing.

The preceding experiment was repeated with tube No. 196, with nearly the same results, the needle first deflecting 40° and then 80° . On further heating the tube, the same sudden intense stratified light appeared, after which the discharge ceased.

No. 187 was then replaced in the circuit, and the same phenomena as already described were obtained.

I now again avail myself of Dr. Robinson's notes of the experiments made on the 5th of August. Tube 190 is of the same dimensions as 187 and 196; but instead of the coke balls, it has brass wires attached to the platinum. In this tube (190) the luminous discharge did not appear until the caustic potassa was heated, when most dazzling strata were observed. Dr. Robinson says,—“I had to use a dark-green glass to examine the strata; as I was observing, the last stratum rolled leisurely away, like a globe of light, from the others, to the negative glow, in which it appeared to dissolve. As the potassa cooled, the strata shrunk up and dissolved at the positive wire, as did the glow; and when the dark negative reached the point, all luminosity ceased.”

On a subsequent occasion the first discharge in the tube (190) exhibited a fine purple negative glow, with two tawny cloud-like stratifications at the positive. This discharge exhibited the fluorescence of the glass tube in a remarkable manner. The circuit was then broken; on again completing it, I was somewhat surprised at finding the luminous discharge was no longer perceptible. I then slightly heated the *tube* with a spirit-lamp; the stratified discharge reappeared; the needle of the galvanometer deflected about 65° , but the leaves of the electrosopes were very slightly affected. On separating the plates of the micrometer-electrometer, sparks passed; the stratifications in 190 became confused, intermingling with each other, and no longer presenting that clearness of definition which I have described.

With tube 202, which is of the same dimensions, with silver in lieu of brass terminal wires, there was not any discharge from the battery until the potash was heated. At first it presented a fine white glow on the negative wire, then one of a tawny red colour on the positive; and on heat being applied, stratifications, as in 190, were observed.

The battery was then connected to the wires of tube 219, the same with which the experiments with Daniell's battery were made. This showed the luminous negative as in fig. 3 A, but more brilliant than with the constant battery—producing bright scintillations at the positive, as if particles of the carbon were fused and thrown off.

By the preceding experiments, I ascertained that a disruptive or spark-discharge could be obtained in air from the nitric-acid, as well as from the water-battery; and that when these discharges were passed through the highly attenuated matter contained in carbonic acid vacua, the same luminous and stratified appearance was produced as by an inductive coil, a proof that whatever may be the cause of the phenomena, it could not arise from any peculiar action of that apparatus.

With the ordinary arrangement of the voltaic battery, in which the insulation of the cells is disregarded, the luminous discharge is usually obtained by completing the circuit, and then separating the

terminal wires or the charcoal points, the length of the arc being in relation to the number of the elements of the battery.

With the water-battery I have never been able to obtain the slightest appearance of an arc-discharge; for whether the terminals of the battery were the plates of my micrometer-electrometer, metallic wires, or charcoal, the result was the same; viz. clearly separated and distinct sparks, which continued until the water in the cells had nearly evaporated. With four hundred (each cell being insulated) of Grove's nitric-acid battery, similar spark-discharges were obtained between the plates of my micrometer-electrometer, without producing the voltaic arc, although by a momentary completion of the circuit it was obtained with great brilliancy.

In carbonic-acid vacuum tubes, and particularly in those in which carbon-balls were inserted, the disruptive, as well as the voltaic arc discharge (under the conditions described), was obtained from the nitric-acid battery; and in both instances the stratified appearance was observed, the difference being, that with the arc-discharge the stratifications were far more vivid and brilliant, and the discharge itself evidently more energetic. With the disruptive discharge, the needle of the galvanometer was but slightly deflected, nor could any apparent chemical action be observed in the battery; but instantly on the production of the arc-discharge, the needle of the galvanometer was strongly deflected, and in all the cells of the battery it was evident that intense chemical action had been produced.

If carbon-balls are attached to the wires of a carbonic-acid vacuum tube, the arc-discharge is obtained from a nitric-acid battery whenever the potash is heated. This process facilitates the discharge, and assists the disintegration of the carbon particles; and these in a minute state of division are subsequently found attached to the sides of the glass. It is these particles which produce the arc-discharge, with its intense vivid light so suddenly observed, with far more brilliant effects than the usual stratified discharge. During its passage the conducting power of the vacuum-tube is found to be greatly enhanced, as shown by the galvanometer, the electroscope, and the intense chemical action in the battery.

The same explanation that I ventured to offer in the Bakerian Lecture for 1858, as to the cause of the stratified discharge arising from the impulses of a force acting on highly attenuated but resisting media, is also applicable to the discharge of the voltaic battery in vacua; while the fact of this discharge, even its full intensity having been now ascertained to be also stratified, leads me to the conclusion, that the ordinary discharge of the voltaic battery, under every condition, is not continuous, but intermittent—that it consists of a series of pulsations or vibrations of greater or lesser velocity, according to the resistance in the chemical or metallic elements of the battery, or the conducting media through which the discharge passes.

March 29, 1860.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

The following communications were read :—

“On the Volumetric Relations of Ozone, and the Action of the Electrical Discharge on Oxygen and other Gases.” By Thomas Andrews, M.D., F.R.S., and P. G. Tait, Esq., M.A.

This paper contains the full details of the authors' experiments on the volumetric changes which occur in the formation of ozone. From three distinct series of experiments, performed by different methods, they show that when ozone is formed from pure oxygen by the action of the electrical discharge, a condensation takes place, as had already been announced in a former Note published in the ‘Proceedings.’ But the condensation is much greater than the earlier experiments of the authors on the expansion by heat of electrolytic ozone had indicated. It is, in fact, so great, that if the allotropic view of the constitution of ozone be correct, the density of that body, as compared with oxygen, would be represented by a number corresponding to the density of a solid or liquid rather than that of a gaseous substance. This conclusion follows necessarily from the authors' experiments, unless it be assumed that when ozone comes into contact with such substances as iodine, or a solution of iodide of potassium, one portion of it is changed back into common oxygen, while the remainder enters into combination, and that these portions are so related to one another, that the expansion due to the one is exactly equal to the contraction arising from the other. For the details of the experiments and of the methods of investigation employed, reference must be made to the original paper.

The second part of the communication is devoted to the action of the silent discharge and of the electrical spark on other gases. Hydrogen and nitrogen undergo no change of volume when exposed to the action of either form of discharge. Cyanogen is readily decomposed by the spark, but presents so great a resistance to the passage of electricity, that the action of the silent discharge can scarcely be observed. Protoxide of nitrogen is readily attacked by both forms of discharge, with increase of volume and formation of nitrogen and hyponitric acid. Deutoxide of nitrogen exhibits the remarkable example of a gas which, under the action either of the silent discharge or of the spark, undergoes, like oxygen, a diminution of volume. It also is resolved into nitrogen and hyponitric acid. Carbonic oxide has given results of great interest; but the nature of the reaction has been only partially investigated. The silent discharge decomposes this gas with production of a substance of a bronze colour on the positive wire. The spark acts differently, destroying, as in the case of oxygen, the greater part of the contraction produced by the silent discharge. The authors are engaged in the further prosecution of this inquiry.

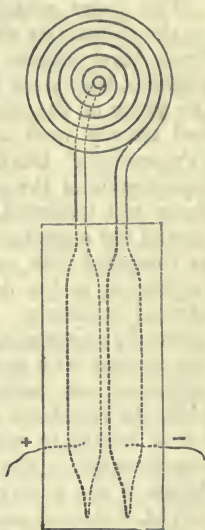
“On the Equation of Differences for an Equation of any Order, and in particular for the Equations of the Orders Two, Three, Four, and Five.” By Arthur Cayley, Esq., F.R.S.

“On the Theory of Elliptic Motion.” By Arthur Cayley, Esq., F.R.S.

“On the Application of Electrical Discharges from the Induction Coil to the purposes of Illumination.” By J. P. Gassiot, Esq., F.R.S.

The subjoined figure represents a carbonic-acid vacuum tube of about $\frac{1}{16}$ of an inch internal diameter, wound in the form of a flattened spiral. The wider ends of the tube, in which the platinum wires are sealed, are 2 inches in length and about $\frac{1}{2}$ an inch in diameter, and are shown by the dotted lines; they are enclosed in a wooden case (indicated by the surrounding entire line), so as to permit only the spiral to be exposed.

When the discharge from a Ruhmkorff's induction apparatus is passed through the vacuum-tube, the spiral becomes intensely luminous, exhibiting a brilliant white light. Mr. Gassiot, who exhibited the experiment at the meeting of the Society, caused the discharge from the induction coil to pass through two miles of copper wire; with the same coil excited so as to give a spark through air of one inch in length, he ascertained that the luminosity in the spiral was not reduced when the discharge passed through 14 miles of No. 32 copper wire.



LXXV. *Intelligence and Miscellaneous Articles.*

ON THE ALLEGED PRACTICE OF ARSENIC-EATING IN STYRIA.

BY DR. H. E. ROSCOE.

PROFESSOR ROSCOE being anxious to obtain further definite information respecting the extraordinary statements of Von Tschudi, quoted by Johnston in his 'Chemistry of Common Life,' that persons in Styria are in the habit of regularly taking doses of arsenious acid, varying in quantity from 2 to 5 grains daily, was supplied, through the kindness of his friend Professor Pebal of Lemberg, with a series of letters written by seventeen medical men of Styria to the government medical inspector at Grätz, concerning the alleged practice. After reviewing the opinions of Dr. Taylor, Mr. Kesteven, and Mr. Heisch upon the subject, and having mentioned the results and conclusions arrived at by those who had previously interested themselves with the subject, Mr. Roscoe stated that all the letters received from the medical men in Styria agree in acknowledging the general prevalence of a belief that certain persons are in the habit of conti-

nually taking arsenic in quantities usually supposed sufficient to produce death. Many of the reporting medical men had no experience of the practice; others describe certain cases of arsenic-eating which have not come under their personal notice, but which they have been told of by trustworthy people whose names are given; whilst others, again, report upon cases which they themselves have observed. Professor Roscoe proceeded to bring forward, in the first place, evidence bearing upon the question, "Is or is not arsenious acid, or arsenic in any other form, well known to, and distributed amongst the people of Styria?" He said that he had received 6 grms. of a white substance forwarded by Professor Gottlieb, of Grätz, accompanied by a certificate from the district judge of Knittelfeld in Styria, stating that this substance was brought to him by a peasant woman, who told him that she had seen her farm-labourer eating it, and that she gave it up to justice to put a stop to so evil a practice. An accurate chemical analysis showed that the substance was pure arsenious acid. Extracts from many of the reports of the medical men were then read, all stating that arsenious acid, called "Hidrach" by the Styrian peasants, is well known and widely distributed in that country. The second question to which Mr. Roscoe sought to obtain an answer was, whether arsenic is or is not regularly taken by persons in Styria in quantities usually supposed to produce immediate death? The most narrowly examined, and therefore the most interesting, case of arsenic-eating is one recorded by Dr. Schäfer. In presence of Dr. Knappe, of Oberzehring, a man thirty years of age and in robust health ate, on the 22nd of February, 1860, a piece of arsenious acid weighing $4\frac{1}{2}$ grains, and on the 23rd another piece weighing $5\frac{1}{2}$ grains. His urine was carefully examined, and shown to contain arsenic; on the 24th he went away in his usual health. He informed Dr. Knappe that he was in the habit of taking the above quantities three or four times each week. A number of other cases, witnessed by the medical men themselves, of persons eating arsenic, were then detailed. Dr. Holler, of Hartberg, says that he and other persons, named in his report, guarantee that they are together acquainted with forty persons who eat arsenic; and Dr. Forcher, of Grätz, gives a list of eleven people in his neighbourhood who indulge in the practice. Professor Roscoe did not think it necessary to translate the reports *in extenso*; he gave extracts containing the portions immediately bearing upon the two questions at issue, and deposited authentic copies of the original reports with the Society for the purpose of reference. He concluded that decisive evidence had, in his opinion, been brought forward, not only to prove that arsenic is well known and widely distributed in Styria, but that it is likewise regularly eaten, for what purpose he did not at the moment investigate, in quantities usually considered sufficient to produce immediate death.—*From the Proceedings of the Manchester Literary and Philosophical Society, October 30, 1860.*

ON THE DENSITY OF MIXTURES OF ALCOHOL AND WATER.

BY M. VON BAUMHAUER.

The author has made a series of determinations of the density of mixtures of alcohol and water. The alcohol was obtained from two different sources, and was rectified first over dried carbonate of potash, and then five times from burnt lime. Thus prepared, one of the specimens had the specific gravity 0.7946 at 15° C., and the other 0.7947 at the same temperature. This number agrees with that found by Pouillet for absolute alcohol prepared by Fremy, and was not changed by further rectification. The mixtures were made in carefully graduated tubes at 15°, and the distilled water was freed from air by continued boiling and cooling *in vacuo*. The alcohol and the water were weighed as a control. The results were reduced to water at its greatest density. They are collated with those obtained by Pouillet, from which they are seen to differ materially.

Alcohol in 100 vols.	Pouillet.	Baumhauer.	
		First series.	Second series.
100	0.7940	0.7939	0.7940
95	8161	8119	8121
90	8339	8283	8283
85	8495	8438	8432
80	8638	8576	8572
75	8772	8708	8708
70	8899	8837	8838
65	9019	8959	8963
60	9133	9079	9081
55	9240	9193	9196
50	9340	9301	9302
45	9432	9394	9400
40	9515	9485	9491
35	9587	9567	9569
30	9648	9635	9636
25	9692	9696
20	9746	9747
15	9799	9800
10	9855	9855
5	9919	9918
0	9901	9991	9991

Comptes Rendus, vol. 1. p. 591.

ON THE THERMAL EFFECTS OF FLUIDS IN MOTION.

BY DR. JOULE AND PROFESSOR W. THOMSON.

In our paper published in the Philosophical Transactions for 1854, we explained the object of our experiments to ascertain the differ-

ence of temperature between the high- and low-pressure sides of a porous plug through which elastic fluids were forced. Our experiments were then limited to air and carbonic acid. With new apparatus, obtained by an allotment from the Government grant, we have been able to determine the thermal effect with various other elastic fluids. The following is a brief summary of our principal results at a low temperature (about 7° Cent.).

Elastic fluid.				Thermal effect per 100 lbs. pressure on the square inch, in degrees Centigrade.
Air.				1·6 Cold.
3·9	Air	+96·1	Hydrogen	0·116 Heat.
7·9	Air	+92·1	Nitrogen	1·772 Cold.
5·1	Air	+94·9	Oxygen	1·936 Cold.
3·5	Air	+96·5	Carbonic acid ..	8·19 Cold.
58·3	Air	+41·7	Hydrogen	0·7 Cold.
62·5	Air	+37·5	Carbonic acid ..	3·486 Cold.
54·6	Nitrogen	+45·4	Oxygen	1·696 Cold.
4·23	Air	{	+46·47 Hydrogen ..	2·848 Cold.
			+49·3 Carbonic acid }	

Further experiments are being made at high temperatures, which show, in the gases in which a cooling effect is found, a decrease of this effect, and an increase of the heating effect in hydrogen. The results at present arrived at indicate invariably that a mixture of gases gives a smaller cooling effect than that deduced from the average of the effects of the pure gases.—*From the Proceedings of the Royal Society for June 14, 1860.*

ON THE TEMPERATURE OF WATER IN THE SPHEROIDAL STATE.

BY C. MARIGNAC.

M. Boutigny's experiments have led to the conclusion that the temperature of bodies in the spheroidal state is constant, and is somewhat below the boiling-point of the body. That of water is 96°·5. Other experimenters, however, have not found so high a temperature.

M. de Luca* considers that the direct determination of the temperature of water in the spheroidal state, by means of a thermometer immersed in the spheroid, is liable to serious errors. He has estimated it by means of a coloured solution, which becomes decolorized at a certain temperature. He used for this purpose iodized starch, the

* *Comptes Rendus*, July 23, 1860.

blue colour of which disappears at 80° , and the decolorization commences even at 50° . When a portion of this liquid is made to pass into the spheroidal state in a platinum crucible strongly heated, it does not become decolorized, and the spheroid retains its colour to the end. According to M. de Luca, this experiment proves that the temperature of water in the spheroidal state does not exceed 80° , and is even below 50° .

M. de Luca also states that a spheroid of albumen containing twice its volume of water becomes opalescent on its surface only, while the centre remains limpid and transparent, so that it can be dissolved in water, coagulated by heat, and the albumen precipitated by alcohol.

The author (says M. Marignac) would probably have obtained more desisive results by placing in suspension in water substances fusible at fixed temperatures. The reactions to which he appeals do not take place at perfectly distinct points; they are possible within considerable ranges of the thermometric scale, but are accelerated by a higher temperature. Those who have had occasion to exhibit the decolorization of iodized starch, must have seen that the temperature reaches boiling before the colour disappears, and it is only decolorized even after some time, although, as M. de Luca says, when exposed for some time to 80° it is also decolorized. The direct measurement of the temperature of the spheroidal state is undoubtedly liable to error from the influence of the radiant heat of the sides of the incandescent crucible. But this affects the stem, and not the bulb of the thermometer, and is therefore inconsiderable.

The following simple experiment gives a method of determining the real temperature of the spheroidal state, not in an absolutely exact manner, but at any rate with a certain approximation, and with complete security.

For this purpose as large a quantity of water as possible is brought into the spheroidal state, and without removing the lamp, the crucible is inclined so that the water falls into a small thin platinum crucible placed at its side, and containing a small thermometer. Another experiment is then made in the same manner, but simply with boiling water.

The temperature of the crucible and the thermometer depend on the mass of the water; and as it would be difficult always to work with the same quantity of water, a crucible should be taken small enough to be completely filled with the water placed in it.

This experiment was repeated several times, and the results have not varied much. With water in the spheroidal state, the thermometer rose to 86° or 87° ; with boiling water it was 90° . These experiments seem to prove that the temperature of water in the spheroidal state is higher than that attributed to it by M. de Luca; they confirm, however, the number admitted by Boutigny.—*Bibliothèque Universelle*, September 20, 1860.

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Fig. 3.

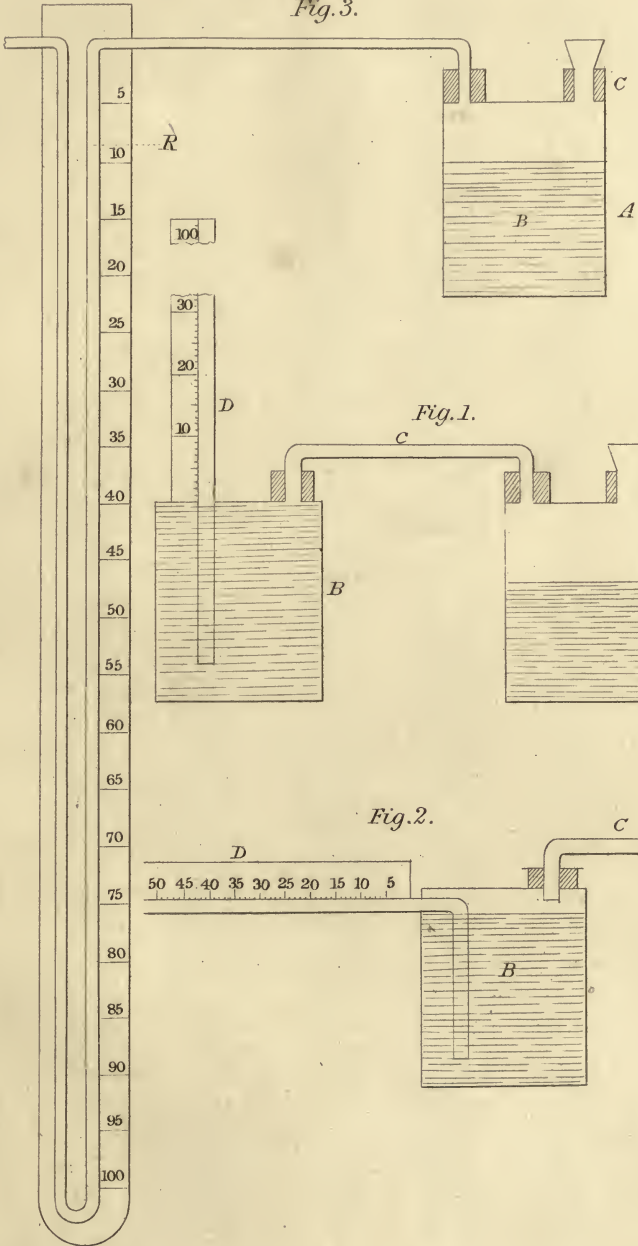


Fig. 1.

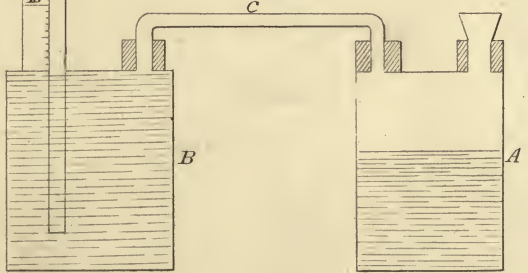
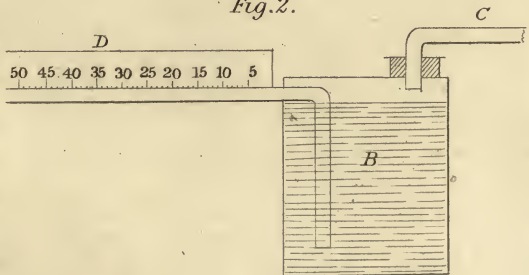


Fig. 2.





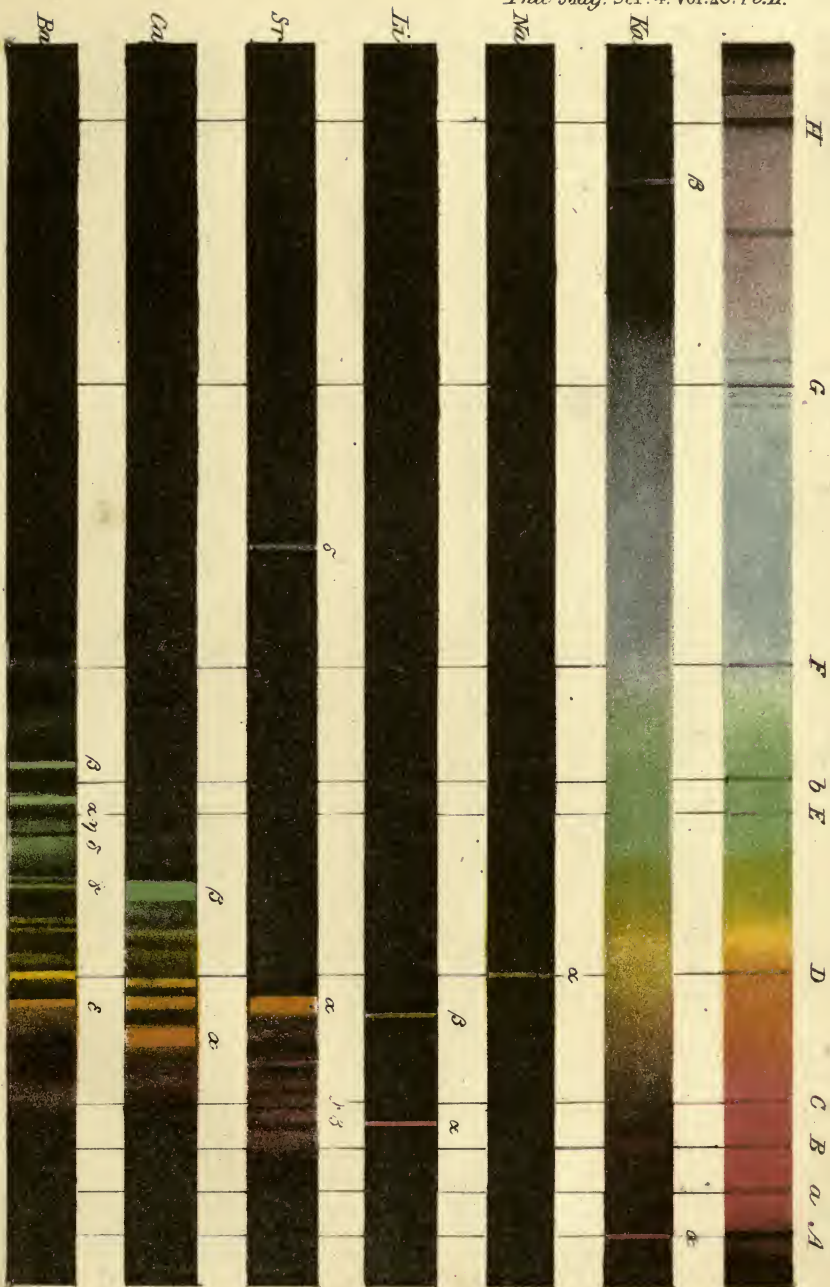




Fig: 1

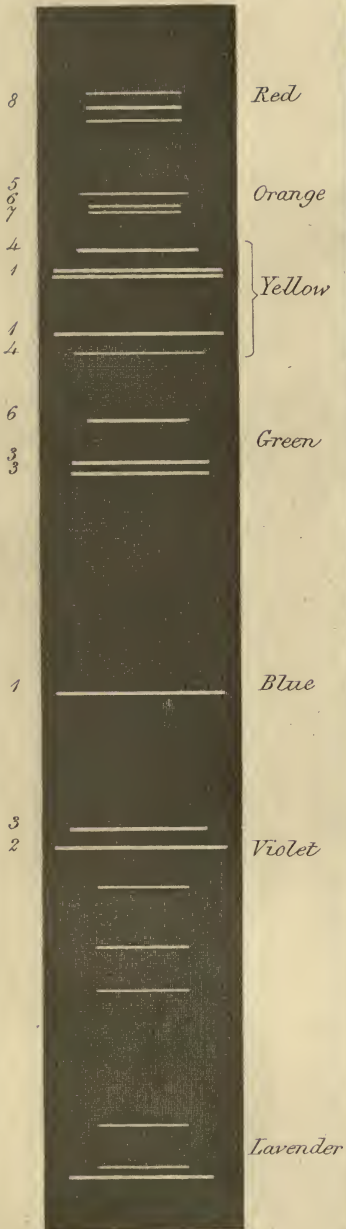


Fig: 2.

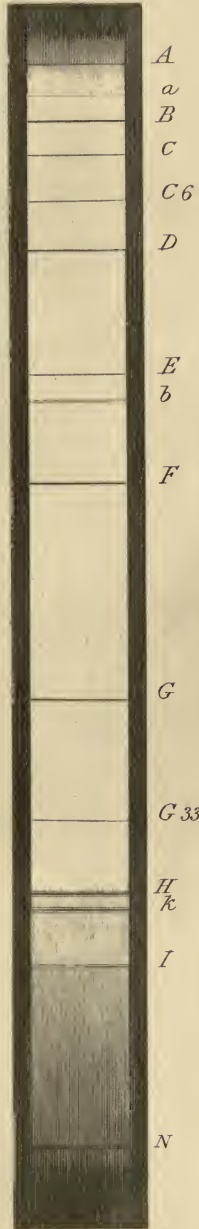


Fig: 3.

